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A Sketch

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CONTENTS

PAGE

I. INTRODUCTORY 1

II. MAIN LINES OF THE HISTORY OF THE PROBLEM IN PRE-EVOLUTIONARY DAYS . . . 8

ARISTOTLE 8

DESCARTES 15

HUME 17

KANT 22

CUVIER 30

JOHANNES MÜLLER 34

III. THE PROBLEM IN MODERN TIMES . . . 37

THE IDEA OF CHANCE 39

DARWIN AND DARWINISM . . . 47

FLUCTUATING VARIATIONS . . . 52

HEREDITARY VARIATIONS . . . 64

DARWIN'S IDEA OF "USEFUL VARIATIONS" . 81

THE MECHANISM OF ADAPTATION . 90

438812

PAGE

IV. "The Exact Sciences" 106

Galileo 107

Newton 111

The Empirical School . . . 115

The Theoretical School . . . 116

Atomic Theories 118

The "Exact" Method 126

V. The Unity of Science 137

Morphological Science . . . 139

Physiological Science 151

The Teleological View of Nature . 158

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I

INTRODUCTORY

NATURALISTS are generally disinclined to occupy themselves at all with philosophical questions. They have a feeling that science is based on mental processes more akin to those of the artist, and that abstract thought can give them nothing positive, nothing new, in their own sphere of work.

Yet it is not difficult to see that the study of nature is everywhere interwoven with philosophical ideas and problems. We encounter them constantly, even when dealing with highly specialised questions in a particular branch of study. Thus, for instance, there has grown up during the past thirty years an extensive literature, the work of zoologists and botanists, in which are discussed such philosophical theories as those of materialism and vitalism.

These philosophical systems represent two opposite interpretations of a fundamental problem. And it is clear, that these interpretations are the direct outcome of the respective methods of thought employed by the opposing schools of scientific workers.

In biology, the greatest interest has centred about the attitude which science should adopt towards the purposiveness of living things. We find in the literature a host of attempts to solve this ancient question or even to prove its solution dependent upon a single series of investigations, or upon a single experiment. Thirty years or so ago, when the experimental modification of the course of development first became the object of extensive and systematic study, and in particular when the phenomena of regeneration came to be known in some detail, such discussions played a great part in contemporary thought. The results of the new experimental work were taken as affording quite new material towards the solution of the philosophical question. Even among philosophers themselves the idea was expressed that it needed only a single series of experiments, of an altogether special character, to solve the question with certainty. Bergson, for instance, in his book, *L'Evolution créatrice*, writes as follows: "It would, then, be possible

to refute the idea of pure mechanism, and in a certain respect provide proof of finality—in the special sense in which we take it—if it could be shown that life at stages of evolution tending in different directions produces homogeneous organs by different means. And the weight of the proof will increase in proportion to the degree of divergence of the lines of development selected, and to the complexity of the homogeneous organs which can be shown to have evolved in them."

It was just the discovery that in development similar results could be reached by different ways that led men like Driesch[1] and Wolff to take up the question of vitalism. For my own part, I had myself at that time special occasion to realise the importance of this problem, being occupied with a series of investigations (on the development of Ascidians[2]) where I likewise found two developmental series leading by altogether different ways to the same result. I found it, however, impossible to concur in the view that any such special investigation could suffice to solve what has rightly been called *the*

[1] *E.g.* Hans Driesch, "Resultate und Probleme der Entwickelungsphysiologie der Thiere," *Ergebnisse der Anatomie*, etc., Bd. viii. 1898.

[2] Johan Hjort, "Germ-layer Studies based upon the Development of Ascidians," *The Norwegian North-Atlantic Expedition, 1876–1878.* Christiania, 1896.

fundamental problem of biology; to wit, the problem of the adaptedness of living beings, or to justify the introduction of the teleological principle into biology. I felt, as Ernst Mach says somewhere, that, sooner or later, *every* student of natural history must face and try to solve the great problem of how to orient his own particular branch of science with respect to the principles of science in general.

Every biologist must find the question of purposiveness one of enormous difficulty. The deeper we go into the problem, the more do we feel that it is really itself a part of the whole great structure which science and thought have erected. A single lifetime would not suffice to master the literature of philosophy, physics, chemistry and biology. An exhaustive exposition of the problem would be an insuperable and time-wasting task for author and reader alike. One must therefore be permitted to select quite arbitrarily the points which appear to oneself as of importance for the exposition of the special ideas which have arisen in one's own mind. To limit my material, then, as strictly as possible, and to put forward my views in the form of a mere outline sketch, seems to me not only justifiable, but actually the best course to pursue, if for no other reason than that it makes no great pretensions, and does not suggest that

the author is aiming at a final pronouncement. It is natural, also, that a biologist should seek his material in the works of those whose ideas of nature have been formed in the course of personal investigation of the facts. Though such works cannot, perhaps, be called philosophical in the technical sense of the word, yet their closeness to nature has meant the inclusion of many valuable thoughts and the statement of many interesting problems. We can, in such works at least, see something of the *difficulties* which are encountered in the course of actual experimental work.

Obviously, also, any philosophical excursus must be made, more or less consciously, from a particular starting-point. And I may here at once state that my own starting-point is the problem formulated by Kant: "How is it possible to arrive at an understanding of Nature?" Kant's entire method shows that philosophical research can have no other material than that offered by the study of nature itself. "Philosophy," says Kant, "is merely a conception of the possibility of a science which is never given in concrete, but which we can seek to approach more nearly by various ways." Philosophy has no other starting-point than a problem and the current results of scientific research; it never leads to any

absolute conclusion. It grows with the science of nature, since in reality it comprises the most general results of that science, and comprises nothing more. It does not explain the nature of the human understanding, and provides no means of getting behind the understanding itself; for philosophy cannot explain that understanding, the existence of which is the first and necessary condition for the existence of science at all. All attempts to do so have led only to movements in a circle, to "building in the sand," and have no other source than "dissatisfaction with the fact that we are only human beings after all."

But science has progressed, and at the present moment there is, perhaps, a particularly rich store of new material available for the investigation of its method.

The marked vagueness still apparent in scientific—and especially in biological—works affords, perhaps, a justification for even this outline sketch as a contribution to the discussion. I have, at any rate, for years past sought in vain throughout the literature I have been able to consult, for any expression of the point of view here set forth.

In a previous work[1] I have briefly indicated

[1] See Sir John Murray and Johan Hjort, *The Depths of the Ocean*, chapter ix. Macmillan, 1912.

the point of view here to be more fully elaborated. Mr. Russell has had the kindness to revise the English of the present volume. For his great help I wish to express my hearty thanks. I wish also to acknowledge the great assistance which I have had from his excellent book, *Form and Function*,[1] which I strongly recommend to all students of Biology, and where the literature—which I have had to disregard in this sketch—may be found by those who wish to go further into the problems here discussed.

J. H.

CAMBRIDGE, *Autumn, 1920.*

[1] E. S. Russell, *Form and Function.* London, John Murray, 1916.

II

MAIN LINES OF THE HISTORY OF THE PROBLEM IN PRE-EVOLUTIONARY DAYS

IT has very frequently been pointed out that a study of ancient Greek literature, not merely of text-books, but of the literature itself, brings to light many of the thoughts of our own day. Natural science has likewise found that many of its profoundest questions have already been put, and solutions attempted, in the literature of ancient Greece. Thus we find in Democritus the leading ideas of the atomic theory, in Archimedes the principles of mechanics, in Plato the first steps towards a critical philosophy, in Thales the monistic idea, and finally in Aristotle the discussion of what I have already called the fundamental problem of biology.

In his work, *De Partibus Animalium*, Aristotle maintains that in all biological study there must be two distinct but complementary points of view. Just as in considering a house it is not sufficient to study the material of which it

is built, but it is necessary also to consider its form, the plan according to which it is put together; so also the student of nature, investigating any part of an animal, cannot rest content with investigating the material alone, but must also aim at explaining the form or plan of the part. As in the case of a work of art, so also in living beings, a distinction must be drawn between matter and form. In nature, indeed, the form and fitness of things is more important even than in a work of art.

"There are then two causes, namely, necessity and the final end. For many things are produced simply as the results of necessity. . . . It is plain then that there are two modes of causation, and that both of these must, so far as possible, be taken into account in explaining the works of nature, or that, at any rate, an attempt must be made to include them both, and that those who fail in this tell us in reality nothing about nature. For primary (*i.e.* final) cause constitutes the nature of an animal much more than does its matter. There are indeed passages in which even Empedocles hits upon this, and following the guidance of fact, finds himself constrained to speak of the ratio (ὁ λόγος) as constituting the essence and real nature of things. Such, for instance, is the case when he explains what is a bone. For he does not

merely describe its material, and say it is this one element, or those two or three elements, or a compound of all the elements, but states the ratio (ὁ λόγος) of their combination."[1]

Matter, which is the source of the accidental, and of blind natural necessity, is a factor in natural existence. In itself, it can only exist as formless and infinite; in space as a potentiality of infinite division, in motion and time as a potentiality of infinite continuance, always only in process of becoming.

We have thus two kinds of causes, the teleological and the material.

I have endeavoured to show in words as near as possible to those of Aristotle himself, how clearly he perceived the fundamental problem of biology stated above. The clear formulation of this is in itself an intellectual achievement of the highest order. In the course of the thousands of years since Aristotle's day, even down to the present time, innumerable biologists have confirmed his view that the teleological must necessarily enter into all biological problems: in botany, zoology, morphology, in physiology and in the various branches of medicine. In text-books of high standing, more especially in physiology, we find

[1] The Works of Aristotle, *De Partibus Animalium*, par. 642*a*. Jn. W. Ogle, Oxford, 1911.

discussions in every chapter concerning the functions of organs, and their importance to the existence and life of the organism as a whole. The biologist who has not himself felt the compelling necessity of adopting the teleological standpoint, will perhaps find it more convincing than a host of quotations, to make the following mental experiment. Let him try, for instance, to imagine an anatomical preparation, say, of an arm, without admitting any teleological considerations. He would then see in the preparation only a red mass of flesh, with some hard masses embedded therein, and a number of differently coloured fibres. Let us now imagine that we are obliged, by our scientific principles, to *describe*, and only to describe, what we see in front of us, without in any way giving scope to ideas of what *purpose*, *function* or *task* the different parts of the preparation might have had in a living state. Such an experiment in thought shows us clearly that we should by such a proceeding divest living nature of all that content which enters into our normal idea of living organisms, and that in reality all the useful ideas which we have in this connection are derived from the concepts of function and purpose. There is this difference between "flesh" on the one hand and "muscle" or "contractile tissue" on

the other, that the former expression tells us, as Aristotle puts it, nothing about nature, whereas the latter opens up a world of ideas and problems. It is the same with the nerves, the blood-vessels, and all the rest of the matter in our preparation. The word "nerve" suggests the idea of impulses towards activity, or feeling; "blood-vessel" is suggestive of the entire nutritive system, and "bone" brings up all those problems of leverage which we meet in mechanics. The introduction of these teleological ideas, then, does not only illuminate our conception of the living thing, but sets a task before us, gives us the desire to construct models with levers and elastic strands in imitation of the bone and muscle system, in order to ascertain how the various "organs" work. The experience gained from such work opens up new and rich possibilities for a *description* of the arm as something actually alive and forming part of a living whole.

Let us take another example. When a zoologist looks over his collections, in a museum or on a scientific expedition, and sets out to describe the inhabitants of the various geographical areas, he soon finds that such description can only be made vital and valuable by correlating the structure of the inhabitants of a particular area with its natural conditions.

These correlations have from early times forced upon naturalists the idea of *adaptation*, and a consideration of the scientific material actually extant shows that this idea presupposes a teleological agreement between the life and well-being of the organism on the one hand, and the natural conditions or environment on the other. This agreement is apparent both in the structure of the animals and in the functions of their separate organs. The recognition of this agreement again suggests a multitude of problems to be solved, as, for instance, the charting of the geographical distribution of animals in relation to certain definite physical factors, and, further, the experimental investigation of the influence of certain factors upon the organism.

It is evident, then, that Greek thought, as we find it in Aristotle, had already grasped in all essentials one of the leading principles of biological research valid for all time. The application of this principle by the Greeks was necessarily incomplete, and intermingled with considerable misunderstanding. But, nevertheless, we find that Aristotle had reached a correct understanding of very many biological functions, and in *De Partibus* he has given us scientific ideas of lasting value.

On the other hand, we cannot be surprised to

find that his principle had the tragic fate of being misapplied under the influence of the whole philosophical tendency of the age. Among the "theological" philosophers, such as Socrates, the general attitude towards the teleological principle was expressed approximately thus. Things in nature appear to show adaptation to a purpose, that is to say, they arise out of *thought* and *selection*. Aristotle tries, it is true, to attain to a higher vision, but he could not avoid connecting the problem of life with the problem of consciousness. He imagined the teleological as something akin to artistic, unreflecting production.[1] He was led, "through his teleology and his consideration of the organic world, to a pantheism which everywhere allows the divine thought to penetrate into matter, and there bring things to actuality in their growth and development."[2]

There is probably no other philosopher whose ideas have played a greater part in the history of humanity, or who has been more misunderstood and abused. Throughout the Middle Ages, when "thought was theological"[3] men laid the chief stress upon his logical system,

[1] H. Brøchner, *Den graeske filosofis historie.* Copenhagen, 1873.

[2] F. A. Lange, *Geschichte des Materialismus*, Bd. i.

[3] Høffding, *Geschichte der neueren Philosophie.* Leipzig, 1895.

his ideas, his words. "The paucity of available facts had to be counterbalanced by a wealth of theorising."[1]

Instead of seeking evidence of the teleological principle in nature, which Aristotle after all did to a great extent, men sought it in the world of thought alone. So it came about that in course of time the teaching of Aristotle became a hindrance to the progress of science. The investigators of the Renaissance, such as Leonardo, Kepler and Galileo, had again to justify the method of "efficient causes. . . ." It is, however, highly interesting to note how the Aristotelian idea nevertheless reappears, as soon as the new science begins to occupy itself with biological problems. We find in Descartes, as also later in the interesting little book of De la Mettrie,[2] what is in reality a restatement of the Aristotelian teleological problem, but in a new form, characterised by the mechanistic philosophy of the Renaissance. As Descartes in his hypothesis of the origin of the world disregards theology, so also in his theory of the organic mechanism he ignores psychology, and conceives the human frame as composed of material parts, acting according to the laws of heat and motion. Descartes

[1] Høffding, *loc. cit.*

[2] *L'homme Machine.*

was therefore long regarded, and the word mechanism itself considered, as opposed to the Aristotelian mode of thought. It is obvious, however, that there can be nothing which more fully implies the teleological view than the concept of a *machine*. Kant has somewhere defined a machine as "that which acts by virtue of its form," and there is after all no difference between the Aristotelian metaphor of the house and its planning on the one hand, and on the other Descartes' comparison of the living thing with a machine. The method of thought is the same. The attitude of the two thinkers towards the psychical was perhaps hardly of such importance in their system of philosophy as their commentators have assumed; possibly it may have been of quite subordinate significance in both, and rather a product of, or a concession to, their environment. The essential point for them, as for pure, undogmatic science, was an understanding of how the investigation of nature should be carried out in order that knowledge of objects might be increased.

From this point of view, the form in which Descartes set forth his problem likewise contains an element which will always be of the greatest importance to science. In modern research, the comparison between living beings

and machines is a very prominent feature. A single example may be taken from one of the best biological text-books of the day.[1] "By cell structure, then, we mean those elements with which, or by whose aid, the work of the cell is to be carried on. . . . If we look upon the cell constituents as chemical compounds merely, without the assistance of some mechanism, nothing but heat could be obtained from an oxidation. The same thing applies to a petrol motor with its fuel. If smashed up and mixed together, nothing but heat would be obtained by burning the mass."

To arrive at a deeper understanding of the Aristotelian problem, however, as also of that of Descartes, the philosopher must have recourse to a criticism of the very idea upon which the problem actually is based, namely, the concept of cause. The problem as viewed by Hume, and later by Kant, was, in its essence, not whether the causes were material or psychical, but what was in reality contained in our ideas about them.

When Hume subjected his own ideas about causes to critical analysis, he found nothing beyond two elements—contiguity and succession. Let us consider the impact of one

[1] W. M. Bayliss, *Principles of General Physiology*. Longmans, Green & Co., London, 1915.

billiard-ball upon the other, and follow Hume's own superlatively lucid exposition: "Having thus discovered or suppos'd the two relations of *contiguity* and *succession* to be essential to causes and effects, I find I am stopt short, and can proceed no farther in considering any single instance of cause and effect."—"When I cast my eye on the known qualities of objects, I immediately discover that the relation of cause and effect depends not in the least on *them*. When I consider their relations, I can find none but those of contiguity and succession." Hume therefore sets up the following definition: "we can define a cause as *an object, followed by another, and where all the objects similar to the first are followed by objects similar to the second.*"

This in itself gives rise to the question whether there is not in the concept of cause itself an idea of necessity. This, however, would be to presuppose that a cause was dependent upon certain qualities in the objects themselves; but Hume could not convince himself that the causal relation did depend upon any such qualities. He therefore sought the psychological explanation of our conception of this necessity, and summed up his conclusions as follows: "But what is there in this whole matter to cause such a strong conception,

except only a present object and customary transition to the idea of another object, which we have been accustomed to conjoin with the former?" *Psychologically*, then, cause may be defined as: "an object followed by another, and whose appearance always conveys the thought to that other."

Clearly, the transition from Aristotle's to Hume's conception must be regarded as one of the greatest revolutions in human thought. In science, it first of all drew attention to the vagueness which is implicit in all "occult qualities." "Virtus movens," "virtus dormitiva" have given way to *relations*, "post hoc non procter hoc"; it has become the task of science to describe the *succession* of objects or phenomena.

Hume's definition of cause includes, as we have seen above, also the idea of time, inasmuch as we have an object (the cause) followed in time by another (the effect). If we imagine cause and effect to be simultaneous, then, says Hume, this "would be no less than the destruction of that succession of causes which we observe in the world; and indeed the utter *annihilation of time*." *Final causes* —that is to say, causes acting retrogressively from the "goal," and in reality implying a movement contrary in direction to that of

time,[1] *are therefore not causes at all, according to Hume's definition of cause*; they can at the utmost be *relations outside all idea of time*, unless they contain merely an indication of a *problem*, a relation not occurring in the relation first established, *i.e.* a relation not yet made clear.

The following observations of Hume's are of the highest importance for the philosophical understanding of biology. "There is, however, another artifice by which we may induce the imagination to advance a step further; and that is, by producing a reference of the parts to each other, and a combination to some *common end* or purpose. A ship, of which a considerable part has been changed by frequent reparations, is still consider'd as the same; nor does the difference of the materials hinder us from ascribing an identity to it. The common end, in which the parts conspire, is the same under all variations, and affords an easy transition of the imagination from one situation of the body to the other.

"But this is still more remarkable when we add a sympathy of parts to their *common end*, and suppose that they bear to each other reciprocal

[1] I have, of course, no intention of entering here upon a discussion of the far-reaching question as to what is "time." I may refer to P. Natora's excellent work, *Die logischen Grundlagen der exakten Wissenschaften*, p. 339. Teubner, 1910.

relations of cause and effect in all their actions and operations. This is the case with all animals and vegetables; where not only the several parts have a reference to some general purpose, but also a mutual dependence on, and connection with each other. The effect of so strong a relation is, that tho' every one must allow, that in a few years both vegetables and animals endure a total change, yet we still attribute identity to them, while their form, size and substance are certainly alter'd."

"From thence it evidently follows, that identity is nothing really belonging to these different perceptions, and uniting them together; but is merely a quality, which we attribute to them, because of the union of their ideas in the imagination, when we reflect upon them. Now the only qualities which can give ideas an union in the imagination, are these three relations above mentioned (resemblance, contiguity and causation). These are the uniting principles in the ideal world, and without them, every distinct object is separable by the mind, and may be separately consider'd, and appears not to have any more connection with any other object, than if disjoin'd by the greatest difference and remoteness."

"In this view, therefore, memory does not mean so much *produce* as *discover* personal

identity, by showing us the relation of cause and effect among our different perceptions."

"Identity depends on the relation of ideas; and these relations produce identity, by means of the easy transition they occasion."

If the reader accepts in the main the reasoning adopted by Hume, he will be obliged to admit, that after this, there can no longer be any question of classing together efficient causes and final causes in science, as Aristotle did, and that the whole question of final causes has *become a problem, not of the object itself, but of the human understanding.*

It is upon this basis that Kant's great biological work, the *Kritik of Judgment*, is built. Kant felt to the fullest extent how indispensable for science is the teleological view; but in entire appreciation of Hume's criticism of the forms this view had taken in Aristotle and Descartes, he endeavoured to establish a definite scientific *method for the application of the teleological view*, and to elucidate the manner in which nature should be judged from that point of view. It is for him, then, a question of acknowledging, on the one hand, the unrestricted supremacy of efficient causes, and, on the other, of explaining how it is that our understanding requires also the aid of the teleological point of view.

"The *privilege of aiming* at a merely mechanical method of explanation of all natural products is in itself quite unlimited; but the *faculty of attaining* thereto is by the constitution of our Understanding, so far as it has to do with things as natural purposes, not only very much limited, but also clearly bounded. For, according to a principle of the Judgment, by this process alone nothing can be accomplished towards an explanation of these things; and consequently the judgment upon such products must always be at the same time subordinated by us to a teleological principle. . . . It is therefore rational, even meritorious, to pursue natural mechanism, in respect of the explanation of natural products, so far as can be done with probability; and if we give up the attempt, it is not because it is impossible *in itself* to meet in this path with the purposiveness of nature, but only because it is impossible *for us* as men. For there would be required for that intuition other than sensuous. . . .

"Hence if the naturalist would not waste his labour he must, in judging of things, the concept of any of which is indubitably established as a natural purpose (organised beings), always lay down as basis an original organisation, which uses that very mechanism in order to produce fresh organised forms or to develop the existing

ones into new shapes."[1] "It is then one thing to say, 'the production of certain things of nature . . . is only possible through a cause which determines itself to action according to design'; and quite another to say, 'I can, *according to the peculiar constitution of my cognitive faculties*, judge concerning the possibility of these things and their production, in no other fashion than by conceiving for this a cause working according to design.'"[2] "The method of representation according to final causes may be only a subjective condition of the use of our Reason, when it not merely wishes to form a judgment upon objects as phenomena, but desires to refer these phenomena, together with their principles, to their supersensible substrate, in order to find certain laws of their unity possible, which it cannot represent to itself except through purposes." "The natural products formerly held to be natural purposes have no other origin than the mechanism of nature."[3]

I have endeavoured in these quotations to give, in his own words, a more or less connected idea of Kant's standpoint. Despite the wearying effect of such quotations, it is only

[1] *Kritik of Judgment*, p. 336. Trans. by J. H. Bernard, London, 1892.

[2] *Loc. cit.*, p. 309.

[3] *Loc. cit.*, p. 351.

by using the actual words of a writer that one can give the reader a true conception of his meaning.

It is thus easy to see what Kant means when he says that "an organised product of nature is one in which every part is reciprocally purpose (end) and means," that "nothing in it is in vain, without purpose, or to be ascribed to a blind mechanism of nature."[1] All this must be understood as being merely a thought-regulating manner of observation, so that "those purposes lie only in the idea of the judging (subject), and not in an effective cause."[2]

One thing, however, which is not clear in the foregoing, is the distinction which in effect is drawn between organised and other objects. This is still more marked in other parts of his book, where he refers to the "objective teleology" of living beings. He states, for instance, that living beings are, after all, given in nature as beings acting according to purpose, which seems "to make the idea of a natural purpose a constitutive principle" (*i.e.* not a regulative principle), "and in this it differs somewhat from all other ideas."

The point which here strikes us as logically in contradiction to his own system is that Kant's

[1] *Kritik of Judgment*, p. 280.
[2] *Loc. cit.*, p. 281.

whole starting-point consists in a theory of understanding which never asks what objects are, but only how they can be judged, "how is any understanding of nature possible?" If, now, there are, in respect of the theory of understanding, two sorts of beings, which must be differently regarded from the teleological point of view, then the entire teleological view must rest, not in the understanding itself, which can only be a unity, but in the object, and it is then hard to perceive any difference between the views of Kant and those of Aristotle.

It is very difficult, however, to penetrate fully into the question of what was Kant's final attitude towards this question of principle. The point might doubtless be discussed *ad infinitum*, without leading to any definite result. Although it is by no means my object to endeavour to solve this problem, I will nevertheless here put forward the opinion I myself have formed after frequent study and repeated consideration of his works throughout many years.

It has struck me that Kant's works contain a series of ideas amounting to genius in regard to the most difficult questions in the science of nature, and more particularly such questions as in his own day formed no part of ordinary scientific discussion. In developing these ideas

of his, Kant has, in reality, made some highly remarkable prophecies of scientific progress in ages far distant from his own. And the remarkable thing about these prophecies is that they are uttered—quite by the way, as it were—with a kind of apology for their being introduced at all; the author himself describes them as being rather a kind of "fairy tale of reason." This is the case, for instance, with his interesting anticipation of the theory of evolution, where he adds that the difference between the various species is probably greater than the theory would seem to require. Not less interesting is his pessimistic utterance to the effect that Chemistry can hardly at any time become a mathematical science (like Newton's physics), though at the same time he suggests in several places that *if* ever it were to become so, it would have to be through the atomic theory.

This self-critical attitude to his own utterances of genius seems to me to show clearly how seriously Kant took his starting-point, that philosophy was not a final or absolute science, but one whose content at any time must be determined by the material then available in the field of natural science. Ideas and logical deductions might push on ahead, as in Kant's speculations as to the origin of species and the

future of mathematical chemistry, but *in Kant's day philosophy could not operate with these, unless it were to become merely a science of words without any practical content.*

From this point of view one can see that Kant, in the many repetitions and renewed discussions in his *Kritik of Judgment*, is in reality struggling with the problem as to *how the teleological view can exist simultaneously with a monistic view of nature*; that is to say, with a view which is the same for living and dead objects alike.

My conviction that this thought, at any rate in some form or another, was vaguely in his mind, is based upon the following three considerations, which themselves raise questions of fundamental importance, not only for biological science, but for the whole science of nature.

1. We find here and there in Kant a demand that even the inorganic sciences should aim at the establishment of "a morphology entirely parallel to the organic." Form, in living beings, is equivalent to what we now call organisation, and it is just on account of this organisation that teleological views arise. Kant doubtless looked for the ground of such teleological views in the science of inorganic objects, but despite certain indications in physics, he

was unable to find principles which "had any claim to be teleological grounds of explanation."

2. On the other hand, Kant has endeavoured to ascertain whether the principles and working methods of physics (mechanics) could not be employed in biology. His ideal in physics is to understand the mechanism and the laws of nature "as completely as if we could produce them ourselves," for we understand completely only that which "we are able to bring about ourselves according to our own ideas. But organisation as the inner purpose of nature infinitely surpasses our power of such artistic production."

3. Nevertheless, Kant does not relinquish his belief that this, to him, insoluble antinomy is due solely to the limitation of our powers, and not to anything in the object itself; as Høffding has expressed it, it is, after all, the same thing which in reality lies at the root of causal relation between things and teleological harmony in nature.

It seems to me evident that these thoughts must at every stage in the history of science be constantly calling for fresh consideration, and in the last section we also shall have occasion to refer to them again.

. . . . . .

In the first decades after Kant we can per-

ceive a marked influence of his thought on the biology of the day. I can here mention, as examples only, the names of Cuvier, Goethe, Johannes Müller and H. Milne-Edwards.

Georges Cuvier was an opponent of those morphologists who took as the aim of that science the mere description of the relative position of organs, without any regard to the functions of the organs and the dependence of the organism as a whole upon the natural conditions of its environment. "This would be," he said, "for the study of living beings, equivalent to shutting one's eyes to that which gives the greatest pleasure and is at the same time the only true foundation of the science, namely, the wonderful instances of agreement, all the correlations so complicated and so perfect, which give each of them its *raison d'être* and its power of continued existence."

"Birds, living in the air, have as much atmospheric air within them as without. Not only is the cellular portion of their lungs very large, but these organs have also sacs or appendages which are prolonged throughout the body. And thus birds breathe, in a given time, a far greater quantity of air in proportion to the volume of their bodies than do quadrupeds. It is doubtless this which gives their muscle fibres so wonderful and immediately effective a

force. It is this which has made their muscles able to exert the motive power required for machines making such violent movements to keep them hovering in the air by simple vibration of the wings."

In such an example as this, we see Cuvier's marvellous power of combining in a single glance the two ideas of form and function; of seeing, like Kant, everything in the organism as at once means and end,—a method of approach which found expression in his famous theory of correlation. "Thus an animal capable only of digesting meat must, if it is not to perish as a species, have the power of spying out its prey, pursuing and grasping it, overcoming it and tearing it asunder. It must then necessarily have keen eyesight, a fine sense of smell, rapidity of movement, and be able to use feet and jaws with swiftness and ease. A sharp tooth, for instance, capable of rending flesh, will never be found in the same species together with a foot enclosed in horny substance, which is only suited for the support of the animal, and cannot be used for grasping. Hence the rule that all hoofed beasts are herbivorous; and those still more detailed rules, which are merely consequences of the first, namely, that hoofs on the foot imply molars with flat crowns, a very long intestinal canal, a capacious and subdivided

stomach, and numerous other features of the sort." This is what Cuvier calls the correlation of functions, and it was in the study of this that he considered the aim of general biology to lie.

This programme of Cuvier's does not take cognisance of the historical point of view; it contains simply the method of combining in thought the form and function of the object of investigation, whether this be an organ or organism, a series of organisms or the entire organic world.

From such a point of view there can naturally be no possible science of biology that does not consider both form and function. But there can, of course, be branches of the science which are more particularly concerned with form, and others more largely concerned with function. Cuvier's idea approaches very nearly to the view which has played a considerable part in physics and chemistry, namely, of regarding everything from the point of view of energy.

From this point of view, *morphology* will be essentially the science of living beings considered as *working machines*; as *living implements in the service of function.* A comparison between organisms will then necessarily view the types of organisation as adaptations to the special work required of them.

Physiology will, according to this principle, become chiefly a study of the actual processes of functional activity, the study of what Claude Bernard has called the "chemical conditions involving and controlling that which appears to us as the vital process."

But we can also imagine a special line of work, the chief interest of which would consist in a comparison between function and form, taking as the starting-point of its *observations* the functional, taking, that is to say, the concept of a function as necessary to the vital process, *e.g.*, the process of respiration, and seeking to formulate what this idea implies in its principal and most general features; endeavouring thereafter to study what kind of machines (organs) are used for the carrying out of this function in the various organisms. This school of thought can in many respects be compared with the physical chemistry of recent times, which, in its explanation of the various processes and activities, disregards as a first step the particularity of the actual substance considered, and proceeds to compare its activity, taking as showing the generally valid schema, with the facts actually observed in different chemical combinations. As an example, I might refer to the general laws of gases and the application of these laws to the different kinds of gases.

No scientific work has ever had a greater influence on the methods and principles of physiology than the famous text-book of Johannes Müller.[1] We find here the teleological point of view accepted as a basis for the study of all the great problems of physiology, but at the same time we also encounter the difficulties, which Kant had to leave unsolved, namely, the understanding of what he called the "objective teleology" of living organisms.

This is, for instance, apparent in the following quotations: "Organic bodies differ from inorganic not only in their composition, but, further, in that the constant activity which works in the living organic substance acts according to the laws of intelligence and purposiveness. And it is just this which is characteristic of the organism. Kant says, the cause of the form of every part of the living body is contained in the whole, while in dead matter every part has its cause in itself. The harmony of the parts which is necessary to the whole cannot exist without the influence of a force which works throughout the whole organism, and is independent of the separate parts. This force exists before the co-ordinated parts of the whole are yet present; these parts are

[1] Johannes Müller, *Handbuch der Physiologie des Menschen*, pp. 17 and 21. Coblenz.

produced by the germinal energy during the development of the embryo."

Johannes Müller here assumes the existence of a special vital force, different from any of the forces in inorganic nature; and this belief was characteristic of much of the biological work of the middle of the last century.

Thus we find in the well-known text-book by Milne-Edwards[1] a standpoint quite similar to that of Johannes Müller expressed in the following words:

"Les êtres vivants ne sont pas soustraits a l'action des forces générales de la Nature, mais ils sont soumis en même temps à l'influence de la vie, qui est aussi une force, et qui leur appartient en propre. C'est la vie qui coordonne les forces chimiques et physiques de façon à produire les phénomènes dont les corps organisés nous offrent le spectacle, mais elle ne s'y substitue pas et n'en arrête pas les effets.

"Le physiologiste doit, par conséquent, étudier avec soin la série des réactions chimiques et des phénomenès physiques dont l'organisme peut être le siège; mais il ne faut pas croire que dans la machine animée tout puisse s'expliquer par le jeu de ces forces, et je dois attacher non moins d'importance à bien

[1] H. Milne-Edwards, *Leçons sur la Physiologie et l'Anatomie comparée*, p. 4. Paris, 1857.

mettre en lumière ce qui dépend de l'influence de la puissance vitale, force sans laquelle aucun être organisé ne pourrait même commencer à exister."

About the same time, however, a great number of ideas arose, which all came more or less into conflict with this vitalistic point of view. We may especially mention the great discoveries resulting in the synthesis of many organic compounds, the law of the conservation of energy, and finally the various theories of evolution. These new facts or points of view were all considered as directly opposed to any vitalistic theory, and so the philosophy of biology developed in the direction of materialism—a development which we will have to consider in the following section.

III

THE PROBLEM IN MODERN TIMES

DURING the last generation or two the philosophy of biology has by no means followed closely the lines laid down by Kant and Cuvier. Just as general philosophy about the middle of last century showed two diverging tendencies, the one idealistic and the other materialistic or positivist, so also about the same time we notice an increasing divergence of views in the philosophy of biology. The split is most noticeable in relation to the theory of evolution.

For the idealistic school, especially Hegel and his followers, the idea of evolution was merely "a classificatory system which was regarded as a divine world of thoughts or images, within which metamorphoses could go on—a condition comparable with that in the mind of the poet—when one image follows another with imperceptible changes." [1]

[1] Høffding, "The Influence of the Conception of Evolution on Modern Philosophy," in *Darwin and Modern Science.* Cambridge University Press, 1910.

Høffding quotes the following interesting passage from Hegel. "Nature," he says, "is to be considered as a *system of stages*, the one necessarily arising from the other, and being the nearest truth of that from which it proceeds, but not in such a way that the one is *naturally* generated by the other; on the contrary, (their connection lies) in the inner idea, which is the ground of Nature. The *metamorphosis* can be ascribed only to the notion of such, because it alone is evolution. . . . It has been a clumsy idea in the older as well as in the newer philosophy of nature to regard the transformation and the transition from one natural form and sphere to a higher as an outward and actual process." [1]

It was only natural that this method of viewing evolution should clash with the view held by the students of natural science themselves. For Darwin and his immediate successors, the principal aim was to prove that evolution was an historical process. Darwin sought to show this by many ways, in which we may perhaps distinguish two main lines: that of *historical* study, and that of *general biology*.

[1] Høffding, "Darwin and Contemporary Philosophy," *loc. cit.*, quoted from Hegel, *Encyclopädie der philosophischen Wissenschaften*, 4th ed., p. 249. Berlin, 1845.

We have seen in the foregoing that Kant hesitated to adopt a belief in historical descent —not as sharing the views later advanced by Hegel, but from a feeling that the stages shown by the available material were too far apart to justify scientifically the establishment of a theory of evolution. Darwin, therefore, tried, looking at the problem from the same point of view, to demonstrate as far as possible *continuous* transitions between one species and another. From a consideration of the wealth of material he had accumulated throughout a long life of work, he was able to state that: "The affinity of different groups, the unity of types of structure, the representative forms through which fœtus passes, the metamorphosis of organs, the abortion of others, cease to be metaphorical expressions and become intelligible facts."

The Idea of Chance

This attempt to discover the greatest possible continuity in transition stages, the greatest number of "missing links," was particularly apt to give the impression that *chance* was the dominant influence in evolution. "*Darwinism*" was founded on the presumption that slight *accidental* variations

occurred which were preserved and accumulated by natural selection, such slight variations being considered to be hereditarily transmissible from generation to generation.

It has been supposed that this idea of the importance of chance in evolution gains great support from the results of the biometrical study of variation, and also from the results of Mendelian crossings.

If a great number of measurements be made of a single "character" in an animal or plant, *e.g.*, measurements of the length of a bean, and the numerical values so obtained be set out in a curve according to Descartes' co-ordinate system, we obtain a figure approaching very closely to what is known as the binomial curve, which is obtained by expanding the expression $(a+b)^n$ (where $a=b=1$) and plotting the results. There is this peculiarity about it, that it presents a perfect symmetry, so that we have to right and left of the apex of the curve—representing the average of the measurements—equal numbers of observations arranged in a similar way (see Fig. 3, p. 70).

It was soon noticed that the curve also applied to many things which were regarded as purely accidental, and Galton constructed an apparatus in which shot allowed to fall at

hazard arranged themselves in a similar ladder curve. It was also observed that the pattern made by the hits of a practised marksman on a target was arranged in like manner. The general conclusion was drawn that variation also arose by chance.

Similar results were apparently given by the results of Mendelian crossings, which showed that the combination of characters in the progeny came about in many cases strictly according to the "laws of chance."

The thesis was, in fact, propounded by the Darwinians that life had originated from dead matter and evolution progressed continuously from the simplest organisms up to the highest, and that the entire course of this evolutionary process was accidental or due to chance. "The teleological is a product of the accidental"—such is the view we find expressed in numerous works on biology.

The most faithful, if not the most critical, of all Darwinians, Ernst Haeckel, states this view explicitly in his famous *Generelle Morphologie.* While all other sciences seek to explain the causes of things, Haeckel complains that those sciences which deal with the forms and origin of living things "seek their explanations in vitalistic pseudo-causes, acting according

to purpose."[1] "The present fundamental principles of the general morphology of organisms attempt for the first time to supersede this unfortunate and altogether erroneous dualism, and that in respect of the provinces of anatomy and evolution-theory. And, at the same time, they will raise the sciences of anatomy and embryology, by mechanico-causal explanation, up to the firm heights attained by monism, in which principle all the other natural sciences have within more or less recent times found their unshakable foundation."

These views were, however, by no means new. The monistic conception was, as we have seen, expressed already in the days of ancient Greece, and Kant himself strove to give it a real content, instead of taking it merely as a logical postulate. The idea of chance as the cause of the purposive can likewise be traced back to ancient Greece. Thus F. A. Lange writes:

"What Darwin, on the basis of a great store of positive knowledge, gave to our time, was given by Empedocles to the thinkers of an-

[1] Ernst Haeckel, *Generelle Morphologie der Organismen.* "Allgemeine Grundzüge der organischen Formenwissenschaft, mechanisch begründet durch die von Charles Darwin reformierte Descendenztheorie." Berlin, 1866.

tiquity, namely, the one striking thought that the purposive survives, because it is in its nature to persist, while the non-purposive has long since disappeared."[1]

Now it is plain that all this mode of thought must be in the sharpest conflict with the critical philosophy founded by Hume and Kant. The accidental, says Kant, is that which we could imagine as non-existent. But nothing can be an object of experience save that which acts by necessity. Thus, in reality, nothing accidental can exist at all. It is only because we are unable to perceive all the antecedents, conditions and relations of an individual object that its existence appears to us as accidental. When we, as human beings, call a thing accidental, it only means that we give up the hope of understanding it. The accidental therefore consists either of things which we do not consider it necessary to heed, or things which we regard as beyond our power to explain. In the physical sciences, those factors are termed accidental which we voluntarily disregard in the course of an investigation, or which we find we have omitted to notice. Kant, however, in his *Kritik of Judgment* calls the teleological "the link whereby our understanding can alone be supposed to find any agreement

[1] F. A. Lange, *Geschichte des Materialismus.*

between the laws of nature and our own power of judgment."

The more we think over the problem with which all evolutionary research is faced, the more we must realise that it is the many unknown factors which, being so numerous, play such an important part. Of the milliards of milliards of organisms which have lived on our planet, the isolated examples which we have been able to review historically can only be regarded as types constituting links in a necessarily incomplete chain of development. We can only draw general conclusions from these types, and in no sense regard them as constituting a complete catalogue of living things in the past.

When we stand face to face with a troubled sea, our mind recoils from the idea of describing each separate wave, its form, and how it proceeds from *all* the natural conditions from which it arises. We can understand the direction of the wave movement; we can choose each single wave as a starting-point for an investigation, and we can attain to an understanding of *wave movements in general.* But we can never attain to an understanding of all the conditions in nature which give rise to the single wave as it occurs in time and space.

It is the realisation of this truth which led

Kant to formulate the conception of a "thing in itself" (*Das Ding an sich*), which amounts to saying that the human understanding can never attain to absolute knowledge, *i.e.*, analyse a thing completely into all its possible elements. A single investigation embraces only a single aspect of an object (for instance, the length of a bean); all the investigations of science together are far from exhausting its ideal and infinite task (Høffding).

From this point of view, then, the normal curve of error may serve as a standard for comparison with our curves of variation in nature, but it can never in itself be regarded as giving an explanation of the phenomena, nor justify the postulate so often met with in biological literature, that the variations are due to chance. The normal curve is an extremely useful guide for the representation of results of measurements. The correspondence between the curve of variations and the normal curve of error may further, as we shall see in the following (see p. 58), give rise to hypothetical ideas concerning the mechanism which produces the variations. But such ideas may also be misleading, as the following remarks show.

The interpretation of the *Origin of Species* by dogmatic "Darwinians," led to the belief that it should be possible to take

any naturally occurring variants, say, for instance, the largest specimens in a collection of plant seeds (plus variants), cultivate them separately, selecting always the largest from each succeeding generation, and thereby finally obtain a larger type of seed. By an extremely careful and critical study of this process, however (the cultivation of "pure lines"), W. Johannsen succeeded in showing that the method, even after many generations, only led back to the production of seed (beans) with the same range of variation and the same average as that originally selected.[1]

In hybridisation experiments also, it was found that the material which showed such a remarkable agreement with "the laws of chance" did not lead to any new development. Moreover, Mendel had already shown that the descendants of a crossing which occurs only once in nature will very soon lose all trace of such crossing, as by the tenth generation, out of 2048 individuals only two specimens will retain the hybrid characters. Left to "chance," then, hybrid forms would disappear; it is only by artificial cultivation, with its systematic selection and protecting care, that they are kept alive.

[1] *Vide, e.g.*, W. Johannsen, *Elemente der exakten Erblichkeitslehre*, 1913; and the exhaustive list of works there given.

Darwin and Darwinism

From all this, the conclusion was drawn by many, strangely enough, that the Darwinian theory of selection was an error, and Darwin's work has in later times been as much attacked as it was admired thirty or forty years ago. Here, however, Darwin's real standpoint was entirely overlooked. To Darwin's mind, the accidental was one of the greatest of riddles. He recognises throughout that in the origin of any organism there must take part numerous factors which can only be called accidental, and it is not difficult to see that these are just the ones *for which he was not able to suggest any further explanation.* This applies especially to variations, those mysteriously arising deviations from the "species type." He is largely at a loss with regard to these, and frequently asks what they can be. He therefore adopted the attitude of regarding these mysterious — and therefore accidental—variations as the *starting-point for further investigations and desisted accordingly from any closer analysis of them.* His theory of natural selection, then, is not by any means concerned with the explanation of variation, but, on the contrary, deals with the question as to whether a "summation" and preservation (through hereditary transmission

and selection) of given variants, can lead to the formation of new species.

He distinguishes expressly between *useful* and *non-useful* variations.

"That the individuals of the same species often differ slightly in the relative lengths of all their parts may be seen in many works of natural history in which careful measurements are given. These slight proportional differences, due to the laws of growth and variation, are not of the slightest use or importance to most species. But it will have been otherwise with the nascent giraffe, considering its probable habits of life; for those individuals which had some one part or several parts of their bodies rather more elongated than usual, would generally have survived. These will have inter-crossed and left offspring, either inheriting the same bodily peculiarities, or with a tendency to vary again in the same manner; whilst the individuals less favoured in the same respects will have been the most liable to perish."

Whatever opinion the reader may hold of this view with regard to the special question considered—a point with which we are not here concerned—it is plain, from this arbitrarily selected example, that Darwin's starting-point was precisely the consideration of what was

useful (*to the organism*). In his treatment of this question, Darwin bases his conclusions, to a greater extent perhaps than any other biological writer, on the consideration of purposiveness. Wherever he perceived a problem to be solved, he takes as his starting-point a quality or a relation in the organism which he considers can be regarded as useful to it in the struggle for life. The idea of the "survival of the fittest" is in itself, of course, a concentrated expression of this point of view.

If I wished to prove this statement of mine, it would be easy to do so by turning to almost any page in the *Origin of Species*. In this magnificent book there is from beginning to end a most wonderful collection of teleological problems, the product of genius and high imaginative power, providing a treasure-store for future biological investigations. I select one passage in particular, because he here says that when the original variations are useful, that is to say purposive, they may give rise to new forms. The passage reads as follows: "Owing to this struggle, variations, however slight, and from whatever cause proceeding, if they be in any degree profitable to the individuals, of a species, in their infinitely complex relations to other organic beings and to their physical conditions of life, will tend to the

preservation of such individuals, and will generally be inherited by the offspring. . . . I have called this principle, by which each slight variation, if useful, is preserved, by the term Natural Selection, in order to mark its relation to man's power of selection."[1]

In order to understand Darwin's task, and the importance of it to science, it is above all necessary to bear constantly in mind the fact that he never considered it his business to prophesy what variations should lead to new and unknown forms. He sought, on the other hand, in the light of his ideas of variation and selection, to elucidate historical events already known in the world of organic beings. *Like that of all other historical research*, his task was to build up a *reconstruction of the past*, not a *construction of the future*. The immortal service he has rendered to science consists in having created an historical science of biology, just as Lyell created a similar science for the geological history of our planet. And it cannot be urged as an objection against Darwin, any more than against Lyell, or against any historian of mankind, that they cannot foretell future events which depend upon so infinitely numerous and *therefore apparently* accidental factors.

[1] *Origin of Species*, 6th ed., p. 49. Murray, 1883.

We can, however, reasonably expect—not of Darwin, for whom, as we have seen, it was merely a starting-point, but of science, that it should take up a definite attitude towards, and attain to an understanding of, the important and interesting problem which lies hidden behind the remarkable agreement between so-called fluctuating variations and hybrid products, and the above mentioned "laws of chance." It is further necessary to arrive at a clear understanding of Darwin's more prescient idea as to "useful variations."

It must, then, first of all be pointed out that these problems can never be solved with any approach to completeness. However far we may carry the analysis, we shall always encounter fresh problems. In this little sketch it is, of course, impossible to go into detail, but it may not be without value to indicate briefly, by concrete examples, some of the ways in which we foresee, that the vague notion called in biological literature chance may be replaced by definite scientific conceptions.

I propose, therefore, in what immediately follows to give some examples illustrating the application of some definite physical and chemical conceptions to some of the great biological problems, such as fluctuating and hereditary

variations and the problem of the mechanism of adaptation.

Fluctuating Variations

In an important work Heincke[1] has considered the binomial curve and the law of error formulated by Gauss in the following interesting words: "This law of error," he says, "may—applied to the variability of individuals belonging to a local type (race, stock, family)—be expressed as follows: For all the individual observations of an object there is a basal, definite, and true magnitude, and in the same way there is a definite true (average, typical) magnitude which underlies each character in all the individuals belonging to a race; and this magnitude must be regarded as an expression of the local conditions of life. Nature gives us always the impression of trying over and over again to produce this true value for the character, but she never succeeds completely. On the contrary, she makes in each single attempt, *e.g.*, in each individual, *a greater* or smaller error. The size and frequency of these errors follow completely the laws of chance, *i.e.* the Gaussian law of error."

[1] Fr. Heincke, *Die Naturgeschichte des Herings.* Abh. des deutschen See-fischereivereins. Berlin, 1898.

There is this difference between the two views as to variation, that while the idea of accidental or chance variations as a rule starts merely from the anatomical, or, so to speak statical, differences between different individuals, the teleological view always leads to a consideration of functional, dynamical, or physiological problems. In the case mentioned by Heincke we find this expressed in the hypothesis that the variations are due to the *local conditions of life.*

Just as chemistry and physics could never be limited to mere historical study, so also in biology, the all-embracing historical interest brought into prominence by Darwin was felt to be a barrier to progress. It led in the 'nineties to opposition being openly expressed against Haeckel and his school. The feeling became more and more common among biologists, that it was impossible to dispense with physiological views, with the consideration of the functions of living things. Even the so-called "historico-causal" method of the pure morphology of the time could not in the long run satisfy the biologist's need to search out *dynamical* causes, any more than the mere description of the succession of events can satisfy the desire of the historian to discover the forces and causes which have affected the course of history.

These views were most clearly expressed by W. Roux, who may be regarded as the founder of "Entwickelungs-mechanik," or, as it should more properly be called, the *physiology of development.* By means of investigation and experiment, and of critical analysis of the existent material, Roux endeavoured to elucidate the activities, functions and vital processes of organisms and organs, and thus to arrive at a dynamic understanding of living beings akin to the understanding which physical and chemical research have attained in the inorganic world.

"Living beings," says Roux, "can at present only be defined with any approach to completeness functionally, that is to say, through characterisation of their activities, for we have an adequate acquaintance with their functions in a general way, though our knowledge of particulars is by no means complete." [1]

Roux's work soon developed into a rapidly growing movement. Dealing both with the adult and with the embryonic organism, there appeared a large number of works,[2] the special method of which consisted in endeavouring to

[1] Quoted from E. S. Russell, *Form and Function.*

[2] See, for instance, the article, "Entwickelungs-mechanik," by C. Herbst, in *Handwörterbuch der Naturwissenschaften*, vol. iii. Jena, 1913.

subject the organism to varying conditions, in such a way as to keep, throughout each experiment, all conditions save one as nearly constant as possible. Keith Lucas has, in an interesting sketch,[1] endeavoured to formulate a programme for such experiments, the aim of which is to test the functions of cells, organs and organisms, especially when subjected to conditions other than the normal, it being taken as an open question, to start with, *whether the normal reactions actually exhaust all the possibilities of reaction in the organism.*

This programme, of course, is directly opposed to the view which sets out from the assumption that all change is accidental. If we bring in all the accidental conditions, we renounce any attempt at finding out the true causal connections. By systematic investigation of one condition after another in their influence on the organism, we at least *aim at* the elimination of the accidental to the greatest possible extent, and move towards the ideal (inaccessible, of course, as any absolute knowledge always must be) of arriving at a conception in which the accidental plays no part.

The characteristic of this method of thinking is, then, that it does not regard the organism

[1] Keith Lucas, "The Evolution of Animal Function," *Science Progress*, Nos. 11 and 14. 1909.

as an isolated thing, or as isolated in our thought from all other objects. On the contrary, *the method always considers the organism in connection with something else, some relation, some factor, some external or internal circumstance.*

From this point of view science has tried, partly through observation of living organisms in their natural surroundings and partly by experiment, to find the causes of fluctuating variations. A few examples will suffice to show that it has been possible experimentally to prove the relation between fluctuating variations and definite physical or chemical factors in the surroundings.

"If the ova of *Strongylocentrotus lividus* be placed in water at about 8° or 25° C. for an hour, or even for a minute at the time of impregnation, the resulting plutei, after eight days' development, are some 4·4 per cent. smaller than those of ova impregnated at from 17° to 22°."[1]

"Larvæ obtained from artificial fertilisations made in the middle of August are about 20 per cent. smaller than those obtained in April,[2]

[1] H. M. Vernon, "The Effect of Environment on the Development of Echinoderm Larvæ: An Experimental Inquiry into the causes of Variation," *Phil. Trans.*, B. vol. 186, pt. ii. London, 1896.

[2] Experiments carried out at Naples.

May, and October, whilst those obtained in June and July are intermediate in size."

"Larvæ allowed to develop in water containing 50 c.c. of distilled water per litre are 15·6 per cent. larger than those grown under normal conditions, and those containing 25 c.c. distilled water per litre 9·5 per cent. larger.

"Larvæ grown in semi-darkness are 2·5 per cent. larger than those grown under normal conditions.

"Larvæ grown in water containing 1 in 70,400 of uric acid are 12·2 per cent. larger than those grown under normal conditions."

"On an average the aboral and oral arms of larvæ grown in water containing 4000 larvæ per litre are respectively 13·4 and 15·9 per cent. shorter than of those in water containing 500 per litre."

We see from these examples, taken from the interesting work of Vernon, that a great number of factors may influence the size of developing animals.

Jacques Loeb[1] has further obtained the theoretically important result, that the *range of variation* may be determined by tempera-

[1] Jacques Loeb, *The Organism as a Whole.* G. T. Putnams, 1916.

ture, for instance in the following experiment, which gives "the latitude of variation" in velocity "for the first segmentation in *Arbacia*," for some selected temperatures.

Temperature. C°	Latitude of Variation expressed in Minutes.
9°	52·5
15°	13·0
20°	9·6

The idea has often struck me, that there must be some relation between fluctuating variations and all such factors as are known to play a rôle in those biological or organic processes which have an *optimum*. In all such cases we can set out the results in a curve markedly approaching the normal curve of error. This is the case in many reactions of organic compounds to chemical or thermal conditions. The coagulation of casein shows, for instance, an optimum rate at a hydrogen-ion concentration of 2×10^{-5}; the rate or intensity of the coagulation varies between the two values 5×10^{-6} and $1{\cdot}6 \times 10^{-4}$, and the variations

within this range are arranged in symmetrical fashion on both sides of the optimum value.[1] Similar relations have been demonstrated in a number of studies of enzymes and other important constituents of the organism.

Figure 1 shows (according to the investiga-

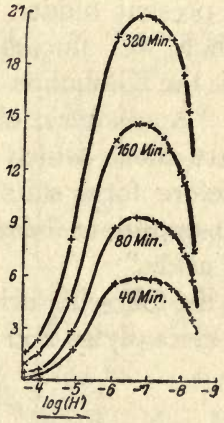


FIG. 1.—Reproduced from Michaelis.

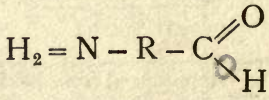
tions of S. P. L. Sörensen), the varying influence of the catalase of the liver on hydrogen peroxide (H_2O_2) at different values of hydrogen-ion concentration. A curve is drawn for the amount of decomposition after the times of 40, 80, 160 and 320 minutes. The optimum

[1] See, for instance, L. Michaelis, *Die Wasserstoffionkoncentration*, p. 190. Berlin, 1914.

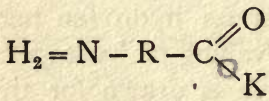
of the influence is in a nearly neutral solution.

In a very important paper, Hardy[1] has developed certain ideas regarding the structure of the proteins belonging to the class of Globulins. These ideas may be destined to form the starting-point for an understanding of the *mechanism*, at present hidden from us behind the phenomenon of fluctuating variations. Hardy regards the Globulins as "amphoteric electrolytes." "Amphoteric electrolytes," says Michaelis,[2] "are those which may appear as acids and therefore form salts with bases, but which may also appear as bases and therefore form salts with acids."

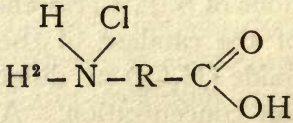
Hardy gives the following schematic formulæ for amphoteric electrolytes and their salts:



the amphoteric electrolyte



the salt with Alkali



the salt with acid

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[1] W. B. Hardy, "Colloidal Solution. The Globulins," *Journal of Physiology*, vol. xxxiii. 1905.

[2] Michaelis, *loc. cit.*

The variation of the qualities of such amphoteric electrolytes when exposed to solutions of varying acidity is illustrated by Fig. 2 from Hardy's paper.[1]

The abscissæ give c.c. of 0·01 normal solution of alkali or acid (added). The ordinates,

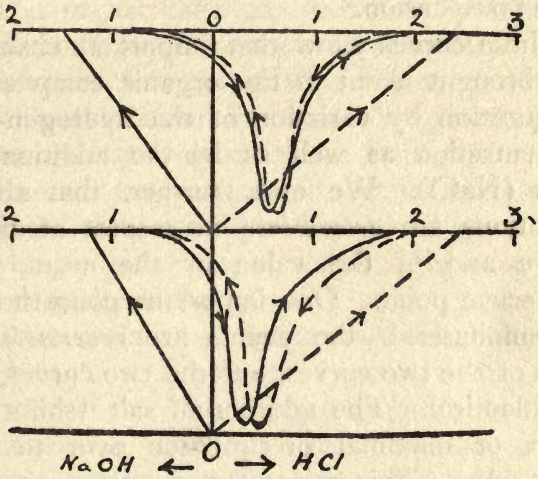

FIG. 2.

the amount of precipitation. Apex of curves, the maximal precipitation.

This figure shows two curves. "In the lower," says Hardy, "starting at O the following events occur in order in the curves;

[1] Hardy, *loc. cit.*, p. 322.

solution by NaOH, precipitation and resolution by HCl, and, in the dotted curve, precipitation and resolution by NaOH. The second precipitation occurs further in the acid region. The other curve traces the same series of events with the same suspension after 20 c.c. 0·02 normal NaCl had been added to 100 c.c. of the suspension."

These curves show that important changes are brought about in the organic compounds in question by variation of the hydrogen-ion concentration as well as by the addition of salts (NaCl). We note, further, that these variations are *determined* in respect of their range and of the value of the mean, the isoelectric point. One important point should be emphasised—the events are *reversible* in each of the two curves, but the two curves are not identical. The addition of salt "shifts the point of maximal precipitation over to the acid side." This raises the question, what is reversible and what irreversible. We may quote in this connection the following remark from Hardy's paper: "When a new state of matter is formed in a system, it may be said to be irreversible *with respect to any one of the factors which produced it.*"[1] "A solid phase, for instance, may become again miscible with a

[1] Italics mine.

fluid phase owing to a rise or fall of temperature, in which case the state is reversible with respect to temperature. . . . The solid phase may disappear when any one of the compounds is added, or removed, as when globulin is redissolved by the addition of water or salt."

If, now, we take the standpoint that variations in living things are no less conditioned by physical influences than are such systems as we have considered above—in globulins, for example—we have at least a standpoint which takes no cognisance of the vague idea of chance, and the hypothesis will give us a starting-point from which to begin critical investigations.

Thousands of investigations of plants and animals have shown, that extensive measurements of selected characters (such as body-dimensions) fall into curves like the normal curve of error (see Fig. 3, p. 70). If these fluctuating variations are to be explained as in the experiments given above or by a mechanism similar to that proposed, it follows as a logical consequence, that the *environmental conditions of the individuals measured must exhibit variations corresponding to the variations shown by the animals.* There must be small deviations from a mean in temperature, light, electrolytes,

nutritive elements or hydrogen-ion concentration in the soil, where plants grow or where animals live. But these variations in the external conditions must, like the variations in the organisms, have a symmetrical range and a definite mean, *i.e.*, the influences on both sides of the mean must be equal. This involves again the consequence that there should exist for each discontinuous *type* of organism a definite set of conditions, so that we, so to speak, must have *two corresponding systems*—an organic one of living types and an inorganic one in surrounding nature.

We have until now considered fluctuating variations quite apart from any question of heredity. We will return to our problem in the following discussion of—

Hereditary Variations

In all the text-books of Heredity which I have happened to see, I have found the "conclusion that two sorts of variations exist, those which are due to the presence of specific factors in the organism, and those which are due to the direct effect of the environment during its lifetime. The former one known as *mutations*, and are inherited according to the

Mendelian scheme, the latter have been termed *fluctuations*, and at present we have no valid reason for supposing that they are ever in-inherited.[1] Regarding the origin of the mutations we find statements like this: "The new variation springs into being by a sudden step, not by a process of gradual and almost imperceptible augmentation. It is not continuous but discontinuous, because it is based upon the presence or absence of some definite factor or factors."[1] "We have begun to realise that each individual has a definite architecture, and that this architecture depends primarily upon the number and variety of the factors that existed in the two gametes that went to its building."[1]

The characteristic attitude of the modern study of heredity is accordingly based on these two points—1. The origin of the mutations, the hereditary variations, is regarded as unknown, and by many, perhaps by most, scientists is looked upon as caused by chance. 2. The starting-point is therefore the factors empirically found in the mutations. "Beyond their existence in the gamete and their mode of transmission, we make no suggestion as to the nature of these factors."

[1] Reginald Grundall Punnett, *Mendelism*. Macmillan, London, 1919.

For an understanding of the Mendelian results it is of value to consider W. Johannsen's remark, that "the study of heredity can never in its experiments do anything more than separate out the different elements. In its study of a stock it may isolate the single individuals, and thereby determine their hereditary qualities. By crossing it may isolate and separate different characters."[1]

In his experiments on *Pisum*, Mendel showed that all the stable combinations which were theoretically possible if seven different characters were observed, really did occur in actual repeated crossings. The number of these combinations was $2^7 = 128$. Mendel thereby proved "that constant characters, which appear in different form in a stock of plants, may unite through repeated artificial fertilisations in all the ways which are possible according to the laws of mathematical combination."[2]

The close correspondence between the results of experiment and the laws of mathematical combination has occasioned much discussion of chance as a factor guiding the combination of the characters present in the fertilised eggs of plants and animals. But a *combined study* of the organisation or structure of the sexual

[1] W. Johannsen, *Arvelighed.* Gyldendal, Copenhagen, 1917.
[2] Translated from Mendel's classical paper.

cells *and* of the experimental results of crossings has led to the conception of the existence in the sexual cells of a special mechanism which occasion combination and separation of factors *by pairs.* The activity of this mechanism is such as necessarily to produce the results found by experiment. We are not here concerned with the evidence on which the belief in such a mechanism is founded, nor with the validity of the interesting theories propounded by Morgan[1] and his school. It suffices for our special purpose to know that such attempts are being made to find a mechanism which will explain the phenomena observed, and it interests us particularly to find the greatest similarity in *method of thought* between the chromosome theories and the molecular theories of chemistry.

In the Mendelian method of hybridisation great care is taken *to eliminate all influence of the surroundings on the result of the experiments,* that is to say, every effort is made to keep all factors constant except those which the crossing itself involves. This procedure follows the well-established principle for experimental work, that only one factor should be varied in one experiment. The success

[1] See, for instance, T. H. Morgan and others, *The Mechanism of Mendelian Heredity.* New York, 1915.

of the work has furthermore confirmed the correctness of the procedure.

As organic chemistry separates and combines *radicals*, of which the highly complicated compounds consist, so the Mendelian method has been able to separate and combine certain units in the organisation of living beings.

But there is a further point of similarity. Among the innumerable compounds which synthetic organic chemistry has been able to discover, there are a great multitude which have never been found in nature.

From this it follows that in both cases we have a *particular* problem before us, namely, which combinations of organic radicals or of biological units can exist in nature, and how these units are separated and combined under the conditions in which living beings must exist. This kind of problem is now coming to the front in organic as well as inorganic chemistry. Why are some compounds, some elements, to be found in nature and not others; why are some of them to be found in greater quantities than others? Under what *conditions* do the different compounds or elements or their different allotropes exist in nature?[1]

The science of heredity has taken up a

[1] See, for instance, U. Kohlschuetter: *Die Erscheinungsformen der Materie* Teubner, Leipzig, 1917.

quite negative standpoint with regard to investigations of this kind. But it is clear that it must be important to consider all the problems connected with the origin and the mechanism of mutations, and with the occurrence of types in nature. None of these problems can safely be disregarded by any student of heredity.

W. Johannsen obtained important results by selecting single specimens of a stock of the common brown bean, which is a self-fertilised plant, and cultivating *each seed separately.* He thus obtained what he calls "pure lines," samples in which each specimen, each individual bean, has the same ancestors (is "homozygote"). If the length measurements of the individuals of each pure line are arranged graphically, they are found to form a curve like the normal curve of error. But there is this difference between the curves from the different pure lines, that they each take up a *different position on the common abscissa.* In Fig. 3, five pure lines are represented by length measurements arranged in the curves A–E, the sixth (the lowest) figure giving the curve resulting from the measurements of all five lines combined. By Johannsen's method a great number of pure lines have been investigated, and cultivation experiments carried out

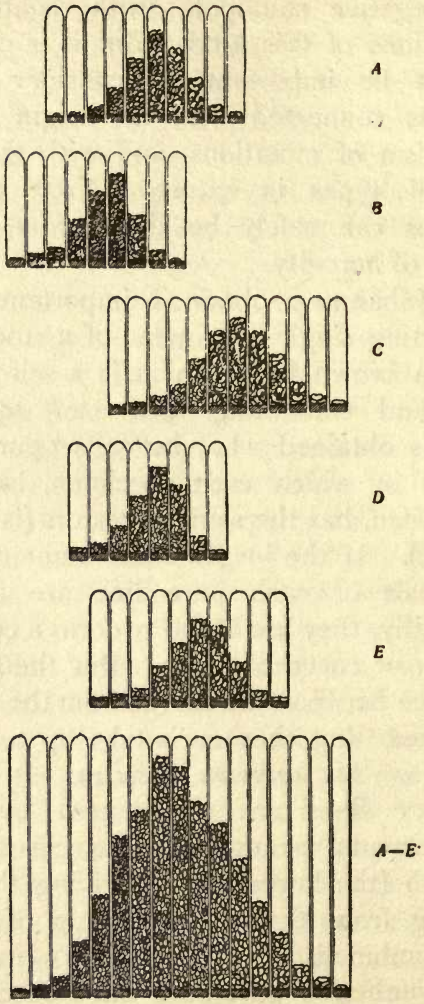

FIG. 3.—*From* Johannsen.

through six years (generations) have shown that the pure lines (their means) are constant in respect of the characters measured.

From these results conclusions have been drawn to support a point of view which is very much in vogue at present, and which goes back more or less to the view of Weismann, *i.e.*, the view that a fundamental and radical difference exists between the somatic and the sexual cells with regard to the influence of external conditions upon them. A distinction has been made between the *phenotype*, which stands for the type of the individuals as modified by the stimuli, to which they have been exposed throughout their lifetime, and the *genotype*, which is supposed to give an idea of the real hereditary qualities. From the genotypical point of view, organisms are considered to be mainly or totally independent of environment of the factors of their surroundings.

There can, however, be no doubt that this distinction made *on principle* between the influence of environment on the somatic and on the sexual cells, originates in a thoroughly dogmatic state of mind. The growth of the gonads in all animals shows the closest correspondence with the growth of the body. Any assumption of a parellelism in this respect

would lead to a quite unacceptable idea of pre-established harmony between the different parts of the body during the whole life of the organism.[1] The experiments of Johannsen have the great and important merit, that they work with material, the "pure lines," in which *the "genotypical" influences may be said to have been kept constant* in the cultivation experiments. Now Johannsen informs us that in this material he has obtained mutations. "The occurrence of mutations *in pure lines* has," he says,[2] "been demonstrated with certainty, and from this it follows that they may arise independently of crossing. When we observe discontinuous variations of the genotype in cross-fertilisation, are we *a priori* unable to feel certain that crossing has no influence on the result." But let us again consider the figure (Fig. 3). We see here that each of the five pure lines form definite curves more or less corresponding to the normal curve of error.

From all we have seen above the conclusion would follow, that the *external conditions* have *not* been constant during these experiments—in any case not to such a degree that fluctuating

[1] Interesting results of the influence of radio-active substances on the sexual cells of insects (Decticus) have been published by L. O. Mohr, *Norsk. Mag. Laegev.*, 1917.

[2] *Arvelighed, loc. cit.*, p. 165.

variations did not occur, or we should be forced to believe that the fluctuating variations were due to hereditary influences, *i.e.* to influences which were not acting during the actual experiment. By external conditions I do here understand all the influences to which the sexual cells are exposed from one generation to the next.

It seems to me, therefore, that the important observations of Johannsen, namely, that mutations do arise when "genotypical" factors are kept constant in pure lines of selfing plants, can best be interpreted in this way, *namely, as indicating the origin of mutations through the influence of external conditions.*

But we may draw the further equally important conclusion that the influence of these external conditions on the organism must be twofold. *Some effects may be traced from generation to generation, and some are only traceable in the generation in which they appear.*

From this point of view I would suggest the use of the words *reversible* and *irreversible* in the way they are defined by Hardy (see above), *Reversible or irreversible from generation to generation with respect to any one of the factors which produced it, or to the conditions under which it was observed.* This would at once bring all the problems into relation with factors,

which are accessible to experiment and would not set up any dogmatic obstacle to research. Let us now consider some other experimental evidence[1] regarding the transmissibility of acquired characters.

1. Amputation of certain parts (the tail of dogs, circumcision in the Jews) has been carried out through many generations and has not had any effect on the descendants. But the argument, that this is a proof against the inheritance of the effect of external influences, reminds us very forcibly of the experiments of Galton, where plusvariants were selected and bred from without any change being effected in the size of the following generations. As we have seen above, no other deduction can be made from such experiments than this, that the *special experiment* had no effect on the equilibrium of the type, and that accordingly not *every* influence may have the power to alter this equilibrium.

2. Another series of experiments is of a different character. Plants from the lowlands have been grown in the mountains, and have developed new characters in the subsequent generations which have been bred in the high-

[1] I here purposely omit most of the experimental evidence, because it would be impossible in this sketch to enter upon the controversies which they have occasioned.

lands. Seeds from these experimentally produced mountain forms have then been sown in the lowlands and have developed into the lowland form. This experiment has been regarded—strangely enough—as a strong proof against the influence of external conditions on the genotypical qualities of the *seeds* (sexual products). But it seems to me that only two conclusions may be drawn from such experiments. Either that the effect on the hereditary factors has been a *reversible* one, or that the so-called reaction norm has been preserved in both cases (when transferred to the highlands and when transferred to the lowlands). Since this was written, Macbride[1] has published some new evidence on this point, to which his attention was recently directed by Professor Dendy: "The peach in Europe is a deciduous tree. Transferred to Réunion it has become an evergreen in the lowlands of that island, but has remained deciduous in the highlands. If seed be taken from the evergreen tree and grown in the highlands, it will still in the first generation give rise to an evergreen tree, although its ancestors were undoubtedly deciduous."

It seems to me that these experiments prove that the influence of external conditions on the sexual products is a problem *special to each case*,

[1] E. W. Macbride, *Nature*, 16th December 1920.

and not a problem to which a general answer is possible. That the influence on the sexual organs should be of a special character and different from the influence on other organs may be fully anticipated.

I should like to ask why it is considered a proof against the influence of environmental conditions on the sexual products, if a plant seed may change first from one to another and then back again to the first type, while it is considered as a proof of hereditary influence if the plant seed has *lost* its ability to return to the type of its ancestors? Does evolution in principle consist merely in *loss of qualities*?

The experience of embryology teaches us that the process of differentiation may go far in *change* of functions without a loss of capabilities proved to exist at stages prior to differentiation. It is in any case a question which can only be resolved by special investigations. All the experience of regeneration and formation of new individuals by budding proves this.

3. Observations as to modifications, made in the course of experiments for other purposes, are becoming more and more numerous. "Evidence continues," says Hopkins,[1] "to accumulate from the biological side to show that,

[1] F. Gowland Hopkins, "The Dynamic Side of Biochemistry." British Association, Birmingham, 1913.

as a matter of fact, the living cell can acquire *de novo* as the result of special stimulation new catalytic agents previously foreign to the organisation.

"It is certain, from the numerous studies made upon the lower organisms, and especially upon bacteria, that the cell may acquire new chemical powers when made to depend upon an unaccustomed nutritive medium. I must be content to quote a single instance out of many. Twort has shown that certain bacteria of the *Colityphosus* group can be trained to split sugars and alcohols which originally they could not split at all. A strain of *B. typhosus*, which after being grown upon a medium containing dulcite had acquired the power of splitting this substance, retained it permanently, even after passage through the body of a guinea-pig and cultivation upon a dulcite-free medium." Very interesting evidence as to the hereditary influence of ultra-violet light on enzymes and bacteria has been given by Victor and Madame Henri and their school.[1]

From the interesting review given of this work I quote the following passage: "Au point de vue biologique ce résultat nous montre que les rayons ultra-violets, en agissant sur les

[1] See a review of the literature on this subject in Madame A. Chauchard's *Thèses*. Paris, 1920.

organismes, peuvent attaquer plus fortement certains ferments que d'autres, et entraîner ainsi un changement dans le metabolisme chez ces organismes. Le mode de nutrition et de transformation des aliments peut donc, dans ces conditions, être modifié d'une façon très profonde, et on conçoit comment cette modification déterminera des effets biologiques importants

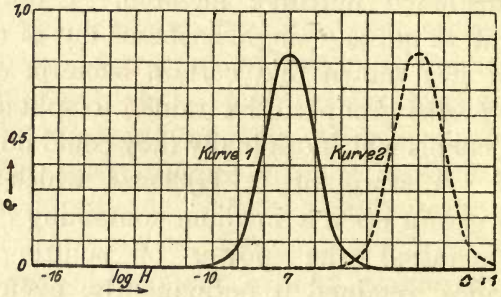

FIG. 4.

pour la vie de la cellule ou de l'être tout entier." We see here a theory of the mechanism by means of which external conditions may be supposed to influence the organisation of living beings, namely, by changing the constitution of the chemical equilibrium existing in them. The same idea may perhaps be illustrated by considering a quite schematical figure, given by Michaelis (see Fig. 4).[1]

[1] See the detailed explanation of the curves in Michaelis, *loc. cit.*, pp. 36–37.

This figure gives the curves for the undissociated part of two different amphoteric electrolytes at different hydrogen-ion concentrations. We see from the figure that chemically different amphoteric electrolytes vary in a similar way (with a similar shape of the curve of variation), but that their curves have a different position on a common abscissa giving the hydrogen-ion concentration. We can from such results imagine a future explanation of the origin of entirely new and irreversible variations, on the supposition that external conditions produce a definite chemical change in the chemical constitution of important components in the cell-equilibria.

. . . . . .

The experimental results and the hypothetical considerations set forth in the foregoing pages warrant the conclusion that science is now well on the way to bring variation into the sphere of rational understanding. It is, nevertheless, necessary to remember that the data to hand are of very limited extent, considering that we here have to do with a principle which should be valid for *all* biological processes for all living organism. This raises the question as to why our experience is still so very limited. It seems to me very easy to give an explanation. First of all, it is clear that evidence to be satis-

factory would have to be drawn from experiments on pure lines. In all other cases the criticism might be made, and rightly, that the experimental evidence had no convincing power because two groups of variables were involved —the external conditions and the influence of hybridisation. This consideration limits the material to "selfing" organisms and organisms produced by vegetative reproduction.

Secondly, the experimental difficulties in varying only one external factor and keeping all others constant, are obviously very great, bearing in mind that the experiments have to be carried out from the beginning of one generation to the beginning of the next. We are therefore at present without any definite evidence of the influence of *special* external conditions in all cases except those in which the succession of generations (as in bacteria) is very rapid.

This much may, however, be concluded from the evidence available, that the influence of external conditions may give rise to mutations, and that the *working hypothesis* may be justified, that all mutations do arise as a result of actions and reactions between external conditions and the living organism.

Great stress has been laid on the fact that new hereditary variations appear *discontinuously*. The word mutation by definition involves the

conception of discontinuous variation. This has been thought irreconcilable with the idea of the influence of external conditions which are supposed to exercise a continuous effect. But modern physiology shows in the clearest way that stimuli may act by summation, the *effect* appearing as discontinuous.

Chemistry as a whole accepts the idea of the discontinuity of compounds. A chemical hypothesis of the origin of new variations would therefore find no difficulty in principle in understanding these interesting differences in factors which Mendelism has discovered.

Darwin's Idea of "Useful Variations"

We have in the preceding pages considered a group of individuals all belonging to the same type, all reacting in the same way to different environmental stimuli, all being exposed to small differences in these external stimuli, and therefore showing minor differences between one another.

The study of living things throughout thousands of years has now established an extensive and minute knowledge of the innumerable types existing in nature, and during the last few generations some data have also been collected as to the correspondence obtaining in

nature between these types of animals or plants and certain groups of environmental conditions. This work should logically result in an approach to the ideal, already mentioned above, that biology should try to build up *a system of conditions in surrounding nature corresponding to the morphological systems of animals and plants.* The old word "habitat," which occurs in all systematic works, is a rudiment of the same idea.

To take an example, I may, for instance, mention that in fishery biology, endeavours have been made to delimit and define the geographical areas of distribution of certain species of fish, and the results show that in this respect there are definite and characteristic differences between allied species.[1]

Such correlations between morphological differences in the organisms considered, and differences in important physical factors in their environment, have always aroused interest in the idea of the *adaptation* of each type to its special group of conditions. We find these ideas already expressed in many of Goethe's writings. His zoological works all testify to his strong belief that "all living beings possess the faculty of adapting themselves to the

[1] See, *e.g.*, *Rapports et procès verbaux du Conseil international*, vol. x. (Copenhagen, 1909), and *Depths of the Ocean.*

manifold conditions presented by external influences, without, however, giving up a certain hard-earned and obstinate independence." In his *Skeletons of Rodents* he says that "the difference of forms is a consequence of their necessary dependence upon the outer world." In his *Introduction to Comparative Anatomy* he attempts to demonstrate the various influences exerted by certain climatic conditions, by water and by air, upon the shape of animals, which becomes altered on passing from one group of conditions to another. This again explains the fact that "no organism adapted to live is conceivable without a perfect organisation."

We can now understand Darwin's conception of useful variations, not as concerned with the question of usefulness "in itself," *i.e.* not as "Darwinism" has understood it, as an origin of the adapted from the unadapted, but as a transition *from one form* (*type*) *of adaptation to another.* "Useful" mutations involve therefore *a relation to the development and the existence of a new species*, not to the preservation of the old type, while fluctuating variations may be regarded as testifying to the stability of the type *as it is.* We shall now realise that adaptation must in a sense bear the same relation to the whole animal as function does to its parts. We shall also immediately understand that the

detailed comparison between animals belonging to such different types must be expressed in different mathematical formulæ from those valid for animals of the same type. To make a biometric comparison between greatly different types it would be necessary, when utilising the measurements of some bodily dimension, to employ some index-value (on the idea originally introduced by A. Retzius for the measurement of skulls of different human types). I have tried myself[1] on these lines to compare the ratio between the diameter of the eye and the length of the head of different species of deep-sea fish with the depth in which the species occurred, and I found that in the deep Atlantic the relative size of the eyes diminished in proportion as the species lived in greater depths, in greater darkness.

To these and similar views, students of heredity reply that science has at present no need of the conception of useful variations. Given a great number of variations, *selection* will decide which shall survive.

In illustration of this dominant view I may quote the following passages from Punnett's excellent text-book: "There are plenty of characters to which it is exceedingly difficult to ascribe any utility, and the ingenuity of the

[1] *Depths of the Ocean*, pp. 686–688.

supporters of this view has often been severely taxed to account for their existence. On the more modern view this difficulty is avoided. The origin of a new variation is independent of natural selection, and, provided that it is not directly harmful, there is no reason why it should not exist. In this way we are released from the burden of discovering a utilitarian motive behind all the multitudinous characters of living organisms. For we now recognise that the function of natural selection is selection and not creation. It has nothing to do with the formation of the new variation. It merely decides whether it is to survive or to be eliminated."

"One of the arguments made use of by supporters of the older view is that drawn from the study of adaptation. Animals and plants are, as a rule, remarkably well adapted to living the life which their surroundings impose upon them, and in some cases this adaptation is exceedingly striking."[1]

One might accept the logic of this point of view if it were carried out, so to speak, to the bitter end. But the fact is that adaptation is accepted in some cases, but not generally—accepted for fluctuating individual variations, for the "phenotypus"—but not for the heredi-

Punnett, *loc. cit.*

tary, or "genotypical," changes. It seems to me evident that no such intermediate course can be taken in dealing with a problem of such fundamental importance as that of adaptation. It must either, as Mr. Punnett says, be supposed to exist "behind all the multitudinous characters of living organisms," or it may be supposed not to exist at all. The argument, that supporters of the theory of adaptation have in a vast number of cases the greatest difficulty in demonstrating the adaptedness of a type or mutation, can hardly carry any weight.

It seems to me of value first of all to consider the consequences of a biological standpoint which professes to make no use of any concept of adaptation. We find such a standpoint expressed in the publications of that brilliant investigator Jacques Loeb. From his book, *The Organism as a Whole*, I take the following example:

Moenkhouse, Loeb and Newman have observed the fact, "that it is possible to fertilize the eggs of each marine bony fish with the sperm of practically every other marine bony fish. The number of teleosts at present in existence is about ten thousand. If we accomplish all possible hybridizations, one hundred million different crosses will result. Of these, only a small fraction of one per cent. can live, and it

is generally the lack of a proper circulation which inhibits them from reaching maturity. It is, therefore, no exaggeration to state that the number of species existing to-day is only an extremely small fraction of those which can and possibly do originate, but which escape our notice and disappear because they cannot live or reproduce. If we consider these facts, we realize that the mere laws of chance are adequate to account for the fact of the apparently purposeful adaptations; as they are adequate to account for the Mendelian numbers."

This point of view is in my opinion of the highest interest, because it shows to what kind of conclusions the one side's experimental view and the belief in "chance" may lead. A study of the life of animals in nature gives us a quite different understanding of the mechanism at work. In nature we find each *species* of fish in the moment of spawning *isolated* from other species either in space or time or in both. We may find the eggs of different species drifting about together, but in the moment of spawning the sexes keep together under quite special and characteristic and environmental conditions. We can therefore hardly avoid the "teleological" explanation, that nature has not developed any mechanism

to prevent such artificial crossings which cannot live, because the mechanism in nature is given in the habit of the fish. The whole reasoning of Loeb therefore does not really tell us anything about what is happening in nature.

But the following examples are still more interesting: "The earlier works explained the growth of the legs in the tadpole of the frog or toad as a case of adaptation to life on land. We know through Guternadch that the growth of the legs can be produced at any time even in the youngest tadpole, which is unable to live on land, by feeding the animal by the thyroid gland. . . . It is quite possible that in nature the legs of the tadpole begin to grow when enough of the thyroid or a similar compound has been formed or is circulating in the animal."

"It might justly be claimed as a case of adaptation, that the egg attaches itself to the wall of the uterus and calls forth the formation of the decidua. We have mentioned the observation of Leo Loeb, that the corpus luteum of the ovary gives off a substance to the blood which alters the tissues of the uterus in such a way that contact with any foreign body (*e.g.* the egg) induces this decidua formation. Again, what appeared as adaptation when unknown turns out to be a result of

the action of a definite chemical substance circulating in the body."

In these quotations we find the idea of adaptation *contrasted* with the idea of physico-chemical explanation. We seem thereby to be forced to a choice between the functional ideal and the physico-chemical ideal of research. Will science necessarily have to abandon its old functional ideas of organs like the legs of a tadpole, in order to be able to accept a chemical mechanism for the development of such organs? Loeb's argumentation is, however, instructive for an understanding of the principles of biology, because it clearly demonstrates that *the negation of the idea of adaptation necessarily leads to the negation of the idea of function.* When, therefore, some students of heredity express the hope that specific difference may ultimately be understood in physiological terms, it seems clear to me, at least, that such a possibility is excluded by the very starting-points of the present science of heredity. Mendelism is at present a purely morphological science, and can therefore have no other working agency than "chance." By its own methods it excludes from its purview the two ideas of function and adaptation. Mendelism is therefore merely a chapter, although a very important chapter, of the science of heredity.

Loeb seems sometimes to have felt this difficulty, since he quite unexpectedly and in the middle of his attacks on adaptation remarks, that *if* the environment influences the organisation in such a way as to "increase its adaptation," this "would not contradict a purely physico-chemical conception of life; it would only call for an explanation of the mechanism by which the adaptation is brought about." And on closer examination of Loeb's own excellent works we find that he in reality is working with an underlying idea of function, and that his difficulty is indeed the *philosophical* problem, how to combine the two ideas: that of function and that of chemical processes.

The Mechanism of Adaptation

To establish a mechanism of adaptation was the great and leading aim of Lamarck. In his *Philosophie Zoologique* (1809) he writes as follows: "The external conditions always and strongly exert their influence on all living beings. This influence is, however, difficult to ascertain, because its effects only appear, and may be recognised, after a very long time." To explain how the external conditions may change the organism, Lamarck established the following

two laws, which I give here in the original form :

"*Première Loi.*—Dans tout animal qui n'a point dépassé le terme de ses développements, l'emploi plus fréquent et soutenu d'un organe quelconque fortifie peu à peu cet organe, le développe, l'agrandit et lui donne une puissance proportionnée à la durée de cet emploi ; tandis que le défaut constant d'usage de tel organe l'affaiblit insensiblement, le détériore, diminue progressivement ses facultés, et finit par le faire disparaître.

"*Deuxième Loi.*—Tout ce que la nature a fait acquérir ou perdre aux individus par l'influence des circonstances où leur race se trouve depuis longtemps exposée et, par conséquent, par l'emploi prédominant de tel organe, ou par celle d'un défaut constant d'usage de telle partie ; elle le conserve par la génération aux nouveaux individus qui en proviennent, pourvu que les changements acquis soient communs aux deux sexes, ou à ceux qui ont produit ces nouveaux individus."

Sir E. Ray Lankester has given the following clear review of the present position of biological science towards these laws :

"The first is universally admitted to be a correct statement of observed facts in numerous cases, though it is not to be accepted as formu-

lating a primary or fundamental property of living matter.

"The second law—stating that 'les changements acquis' (for which words, in the exact sense in which Lamarck used them, the English term 'acquired characters' was many years ago adopted and established as an equivalent) are preserved by generation to the new individuals born from the parents which have acquired those changes in accordance with the first law — has been challenged by many naturalists and accepted by others. Lamarck makes it abundantly clear that the characters transmitted to a new generation—with which he is concerned — are *changes* in form and structure acquired by the parents as a result of *more frequent* and *sustained use* of some organ, or of *constant disuse* of such organ, resulting from the influence of circumstances to which their race has been for a long time exposed."

"They are *novelties* which arise under *novel* conditions, departures from the normal which occur when the environment ceases to be — *in certain important particulars*—that which is associated with the normal form of the species."

"It has been demonstrated that such changes do occur, but no satisfactory evidence of their

transmission by generation to progeny has been produced. It is admitted that, so far as we know, such a transmission is *possible*, and, in the period at which Lamarck wrote, the assumption that such transmission occurs was a reasonable one. But hitherto all attempts to give convincing demonstration of its occurrence have failed, though such attempts have been, and still are, made by able biologists." [1]

We find in this review the same ideas which we have considered above: the distinction between an influence on the phenotypical and the genotypical qualities of living organisms and the demand for experimental proof. As I have pointed out above, such experimental proof has begun to come forward, and it may therefore be a question for serious consideration whether science is not already—in any case in principle — in possession of experimental evidence in favour of a transmission of acquired characters.

It is not the aim of this little sketch to treat of this strictly biological evidence. It may safely be said that the experimental evidence available is as yet very limited in comparison with the enormous scope and range of the Lamarckian laws should we accept them as

[1] E. Ray Lankester, "Heredity and Acquired Characters," *Nature*, 16th Dec. 1920.

valid, since these laws will have to be considered as valid in all cases or in none.

We have seen above that evolution will never be proved as a continuous process. It will have to be accepted or rejected as a general idea or theory. The same will necessarily be the case with regard to the ideas of Lamarck.

The important question, which this sketch cannot pass by, is therefore the question whether the Lamarckian laws contain any elements of thought contradictory of the principles of science, and whether the *experimental* modification of species is the only evidence which justifies us in adopting the theory of adaptation.

The ideas of Lamarck were for a long time, and are still, in the minds of many investigators, closely associated with metaphysical assumptions, such as the belief in special "factors" or "forces," which cannot be clearly defined and the activities of which can neither be measured nor in any way demonstrated. But the concepts of the use and disuse of organs have in the course of the last generations been remodelled in connection with an ever-increasing mass of physiological data, which opens up the possibility of understanding the influence of the use of an organ on the constitution or organisation of the whole animal.

W. Roux, whose great importance to embryology we have considered above, and who introduced physiological ideas and methods into the pure morphology of the 'nineties, also tried to translate the Lamarckian ideas into modern language. Russell gives a most excellent review of Roux's work and ideas, and I may refer the reader to the relevant chapter in his *Form and Function.* Russell says here about Roux's views: "The real thing in organisation is not form, but activity. It is in this return to Cuvierian or functional attitude to the problems of form that we hold Roux's greatest service to biology to consist." *The great change in the method of thought, the transmission from the statical to the dynamical point of view, which embryology has already undergone in our lifetime, will have to come about in the science of heredity also.*

Amongst the great and increasing literature, I may refer the reader to the papers by Fletcher and Hopkins[1] and to those of August Krogh,[2] where we will see, that science is now not only on the way to establishing the mechanism of the effect of the use of organs (especially in the

[1] W. M. Fletcher and F. G. Hopkins, "Croonian Lecture," *Proceedings of the Royal Society*, B. vol. 89. 1917.

[2] August Krogh, *The Journal of Physiology*, vol. lii., No. 6. 1919.

case of the muscular or executive system), but is also approaching an understanding of the influence of this muscular system on the circulation and on the chemical composition of the blood in the other organs (also in the sexual organs) of the whole body. There are therefore no longer any "hidden qualities" behind the assumption of a reciprocal relation between the whole organism and its parts. The interesting discovery of hormones gives such ideas a further positive and quite material basis. Experience derived from the study of living animals in nature has also brought to light most interesting facts as to the great and infinitely varied amount of work which animals do. Marking experiments with cod in Norwegian waters have, for instance, shown that the large fully grown fish swim several thousand kilometres a year.

But besides all such considerations, it is important in this connection to consider the insight which Cuvier has given us into the method of thinking, which he followed throughout his masterly investigations. As mentioned above, the historical aspect did not enter into Cuvier's considerations; his method was to establish relations between the organisation of the bodies of animals and the function and behaviour of these animals in nature. "Since

these relations are constant," he says,[1] "they must necessarily have an adequate cause; but as we are ignorant of this cause, observation must help out theory; observation determines empirical laws, which are almost as sure as rational laws, when based upon a sufficient amount of observations. . . . But that there exist all the same hidden grounds for all these relations, becomes partly apparent from observation itself independent of any general philosophy."

The extensive international investigations of the physics and biology of the sea have brought to our knowledge innumerable such relations between environmental conditions and the distribution and life-history of animals in North European waters. Impressed by the importance of these results, I had for long had the idea that confirmatory evidence almost equal to that to be derived from experiment would be afforded us if it were possible to find another field of observation geographically separated from the North European waters, having the same species of animals and similar conditions in the sea, and then to compare the two geographical areas. Such an opportunity was given to me by an invitation to investi-

[1] "Ossemens fossiles," i. p. 63, quoted from Russell's *Form and Function.*

gate Canadian waters.[1] Here material was collected which proved that the distribution of animals, and, in particular, the spawning areas of different species of fish in their relations to the conditions of the sea, corresponded in many respects to the state of affairs found in European waters. In our European waters we can distinguish between a great number of "growth types" of different animals, for instance, the herring, — one in Norwegian waters, one or several in the North Sea, and one in the Atlantic coast waters. The investigations in Canadian waters proved that they also contain "a system of growth types" very similar to that of the European seas, namely, a Newfoundland type very similar to the Norwegian one, a Nova Scotian or Atlantic type very similar to the type inhabiting the Irish Atlantic coast, and probably several types of the Gulf of St. Lawrence to which similarities may be found in the North Sea or in the waters between the North Sea and the Baltic.[2] The similarity between Arctic and Antarctic species gives us another important instance of two great corresponding systems.

[1] *The Canadian Fisheries Expedition.* Ottawa, 1919. See my introductory remarks and the excellent papers contained in the report, especially by Dannevig, Huntsman and Lea.

[2] Johan Hjort, *Investigations into the Natural History of the Herring in the Atlantic Waters of Canada.* Ottawa, 1915.

In an admirable paper Johs. Schmidt,[1] using the methods of Heincke,[2] has investigated the variation of the racial characters of *Zoarces viviparus* in different waters. He here remarks as follows: "I have, indeed, myself been able, by experiments which I hope on a later occasion to describe further, to prove with certainty in several cases that a change of environment was capable of bringing about alterations in the structural conditions not only of Zoarces, but also of other species of fish. And, as already mentioned, numerous observations of natural conditions seem to point in the same direction. I merely wish to point out that we are hardly justified in regarding all the racial differences which have in course of time been shown to exist among fish species, as exclusively due to the effect of environment. Everything seems to suggest that they should be regarded as the result of co-operation between certain 'internal' and 'external' factors; in other words, that they are of both genotypical and phenotypical nature."

From the point of view I have tried to explain above, this would mean that in a race, stock or species we have both reversible and

[1] Johs. Schmidt, "Racial Investigations," *Comptes-rendues destravaux du Laboratoire de Carlsberg*, 13me vol. 1917.

[2] Heincke, *loc. cit.*

irreversible or *historical* influences. In order to arrive at a clearly defined *method*, studying the influence of external conditions and of studying adaptation itself, it is, however, of great importance to attain to a general understanding of the following points!

An investigator has the right himself to fix upon any discontinuous group for the purpose of comparison with the external conditions to which this special group is exposed. The group may be a family, a genus, a species, a race or a pure line. In this way Cuvier described the adaptation of birds in general, or carnivorous or herbivorous animals. The larger the group chosen the more apparent in most cases will be the adaptation. As systematic biology has analysed the large groups into smaller and smaller, the external conditions of animals will have to be analysed more and more in detail to explain the difference in adaptation between "smaller groups." It should not be a matter for surprise that such an analysis at present is far from complete, for this kind of study is in its infancy. In illustration we might adduce the fact that systematic zoology in Cuvier's time had not much more to say about the difference between herring and sprats than that there were some small anatomical differences (teeth on the vomer, etc.).

The discovery of the pelagic eggs of the sprat (Hensen), and of its spawning in the southern North Sea in summer time, while the Norwegian herring, for instance, has demersal eggs and spawns in deep water in the coldest part of the winter, demonstrated other and remarkable differences. Still more striking, perhaps, was the fact that a sprat of fifteen centimetres in length might be six or seven years old and near the close of its life (Oscar Sund), while the Norwegian herring of fifteen centimetres is only in its second year, and may grow to more than the double length, and live to an age of about twenty years before it dies. But here again the analysis might be carried much further (*e.g.* by considering the separate races and growth-types), according as the extent of our knowledge increases and more problems are raised. A similar extension of our knowledge of the life-history of animals has in all branches of zoology given a new and richer content to the conception of species. It is really along such lines that we may hope to discover the physiological significance of specific differences.

The interesting ideas of Henderson[1] have opened up a new line of investigation, namely,

[1] Lawrence Henderson, *The Fitness of the Environment.* New York, 1913.

the study of the fitness of the environment itself for the life of the organism. This would entail an intensive analysis of the mechanism by which the environment influences the organism. Such investigation would correspond to the physiological investigation of the influence of changes in the blood on the different organs. We have here a perfect parallel between the two great branches of biology, physiology and the study of adaptations to environment.

The following may serve as an example of this. The Ocean absorbs carbonic acid from the atmosphere. This carbonic acid is used by plants for building up their protoplasm. It is therefore a fact of the greatest importance that the ocean contains the so-called buffer salts which, on the one hand, may bind the carbonic acid (as bicarbonate), and, on the other, keep the reaction of the sea water constant within wide limits.

Recent investigations have further shown that the quantitative occurrence of carbonic acid (slowly) alters the reaction, so that it may be possible to determine the carbonic acid tension in sea water by a determination of the alkalinity (the hydrogen-ion concentration) of the sea water.[1]

[1] See L. Henderson and Edwin J. Cohn, "The Equilibrium between Acids and Bases in Sea Water," *Proceedings Nat. Ac. Sc.*, vol. ii. 1916.

Now actual demonstrations of the alkalinity of sea water in Norwegian waters have shown[1] that values may occur which correspond to a carbonic-acid pressure no less than twenty times as great as the amount which the carbonic acid present in the atmosphere might possibly produce.

This excess of carbonic acid must to a very large extent result from the respiration (*i.e.* the work) of the animals in the sea. In the fjords of Norway we may therefore find conditions of life which vary in respect of certain factors which we know from the experiments of Loeb and others to have the greatest influence on the development of organisms.

We may have here one of the many factors which characterise the peculiarly favourable conditions for development of plant life in the coastal waters as compared with the open ocean. The animals may feed on detritus from the land, as Sir John Murray and G. G. Joh. Petersen have shown, and so produce carbonic acid (and nitrogen), and this carbonic acid may again give rise to plant life. But the excessive development of carbonic acid—especially in the deeper water-layers which are shut off from circulation—may produce even an acid reaction,

[1] Thorbjörn Gaarder, *Die Hydroxylzahl des Meervassers Bergens Museums.* Aarbok, 1916–1917.

which may even go so far as to render any animal life impossible. In several fjords, for instance, an arm of the Christiania fjord, the Drammensfjord, the process may even go so far as to bring about a condition in which the deeper layers contain no oxygen and great amounts of H_2S, making the existence of all other life than that of bacteria an impossibility.

The respiration of animals may therefore change the environment to a degree which renders it unfit to support life. Along a line from the open ocean to the innermost part of a Norwegian fjord we find, therefore, a series of different conditions and different organisms. A comparison between these cannot be based on the idea of greater or smaller degree of fitness, but of adaptation to different conditions and of failure of adaptation. From a philosophical point of view fitness and non-fitness, the useful and the non-useful, are the two sides of the teleological point of view in the extreme example we just have considered, as well as in the use of fluctuating variations which follow the Gaussian law of error. The reverse to the teleological is not the non-useful—but *chance.*

The examples I have given in the foregoing pages may perhaps serve to elucidate various philosophical thoughts and ideas which in the course of time have played a very prominent

rôle in biology. They are naturally altogether insufficient for any historical review of the development of biological philosophy. This is apparent also from the fact that they have been chosen from the limited field of interest which happens to be my own. All biologists know that similar examples may be found in all branches of biology and medicine, none, perhaps, more striking than those derived from the study of the habits and life-history of insects.

For the philosophical understanding of the biological method of thought it will be necessary first to consider the methods of thought which are used by physics and chemistry. We will thereby be in a position to consider whether these two "exact" sciences work with principles different from those which have been used to such an extent in biology.

IV

"THE EXACT SCIENCES"

STUDY of the thought of the ancients clearly indicates that biology is the oldest of all the sciences. The earliest human explanations of striking events are based on the conception of ideal beings, the gods, acting with forces and powers, imagined upon the analogy of the powers of the human body. The principles of primitive physics, more especially the fundamental principle of force, are also derived from experience of the powers of the human or animal body. And the knowledge of the ancients in the field of biology was far more extensive than their knowledge of physics, not to speak of chemistry.

The origin of an independent science of physics must presumably be sought in the schools of the atomists, whose chief representative was Democritus, and of the "mechanical" philosophers, represented by Archimedes. Democritus foreshadows the constructive efforts of our own day towards an

explanation of phenomena in terms of the structure of matter. It was probably with a view to explaining the elementary known facts regarding compressibility and elasticity that Democritus established the concepts of atoms and empty space. In Archimedes we find attempts stamped with all the marks of genius to create a science which should explain natural phenomena by *analogies* from the science of mathematics, which was already so highly developed in ancient times.

It was, as we know, on Archimedes' method of thought that the great thinkers of the Renaissance based their new physical science. To my mind the most wonderful thing about this age as a whole is that it could bring to light, in so short a time, all those principles which we recognise even to this day as the basis of scientific method. Leonardo da Vinci, Kepler, Galileo and Bacon saw and thought out all the most fundamental principles of science.

It was at once clear to them that their new method must be opposed to the Aristotelian system, with its efficient causes and final causes. We find in Leonardo the demand for a recognition of necessity; in Kepler, for the recognition of physical causes (*veræ causæ*). Galileo stood out, as Høffding aptly says,

against the formal logic "which, though it may regulate and correct the process of thought, is not a means in itself to the discovery of new truths. Such a discovery—he says—is arrived at by basing a theory on certain known facts, and then seeking deductively to show that the theory so formed agrees with other facts. The analytical method (*metodo risolutivo*) and the synthetic method (*metodo compositivo*) thus reciprocally supplement each other."[1] This passage gives us, in concentrated form, a description of the method of physics. But in applying it difficulties soon appeared which led to two different views of scientific method. The two schools which originated in this way have their representatives even to-day; and in the ordinary scientific mind there is an idea that the one is represented by Galileo and the other by Bacon. How far this may be historically correct, I am unable to judge, but there is no necessity for us here to come to a decision on the point. The important thing is that we should seek to arrive at an understanding of the two schools of thought, and of the problem which must be solved in order to bring them into harmony one with the other. Høffding has aptly said that Galileo's attitude was more particularly associated with mathematics than

[1] Høffding, *Gesch. der neueren Philosophie.*

was Bacon's, and it can hardly be questioned that such is the case.

Galileo deduced beforehand, and altogether *a priori* (see, further, Høffding's excellent exposition of this), the law that falling bodies move with evenly increasing speed through a course, the length of which is as the square of the time it takes to transverse it. Having in his mind deduced this law, he proceeded by induction, *i.e.* by experiments on an inclined plane, to show that the law he had established could be verified. In such experiments the investigator discovers that there are certain difficulties encountered in nature when we attempt to establish an entire agreement between the results of the two methods. Experiments give only an approximation towards the strict mathematical law formulated by deduction. The expectation of entire agreement between the laws set up by the human mind and the play of natural forces can never, therefore, be entirely fulfilled by experiment; it is based upon the conception of an ideal nature, that is to say, of nature simplified.

The discrepancies can in some cases be explained on the grounds of imperfection in our experimental methods.

But in other cases experience has shown that our mental activity may result in the formula-

tion of altogether erroneous laws, lacking any confirmation in nature, while, on the other hand, we have learned by experience also that nature brings us face to face with many highly important facts which we cannot express in any mathematical form. It is therefore easy to understand that a school of thought arose which attached the chief importance to the collection of facts, and held that the ideal of all investigation of nature must be to make as few suppositions as possible beforehand. This school has been called the inductive, and is rightly or wrongly associated with the name of Bacon.

The problem which lies at the base of these diverging lines of thought is undoubtedly the problem of the proper understanding of mathematics. For men like Galileo, the mathematical formula is the eternally true law; such men represent a scientific idealism, since for them truth is represented by the idea. With the adherents of pure induction, it is experience which alone is truth, and their laws have therefore been called empirical, and often materialistic. But here also, as in biology, there is likewise a third point of view, to wit, the critical, which sees only "an analogy between mathematical and physical conditions, just as there is an analogy between logical explanation

and causal relation. It is the justification of this analogy that furnishes the support for mathematical physics, or rather, the justification of this analogy is the great hypothesis which all the modern science of nature seeks to prove in individual cases."[1] Kant called this hypothesis the hypothesis of Nature's formal teleology—the belief, that is to say, that nature and our understanding are both so constituted that we are able to make valid observations with the powers we possess—our power of constructing ideas and mathematical models, and our power of noting and collecting facts perceptible in nature.

It will be useful, in the interest of what follows, to consider some examples of the way in which these different views have cropped up in the discussion of some important physical problems.

It has often been said that Newton's *Principia* is a model of the inductive method. This belief has perhaps been influenced by his famous dictum: "Hypotheses non fingo."

Duhem,[2] for instance, writes with respect to Newton's law of gravitation as follows: "The experimental laws established by Kepler afford

[1] Høffding, "Totalitet som Kategori," *Videnskabsselskabets Skrifter*. Copenhagen, 1918.

[2] P. Duhem, *La théorie physique, son objet et sa structure*, pp. 312 ff. Chevalier et Rivière, Paris, 1906.

knowledge of all the characters presented by the solar attraction of a planet. By induction, Newton generalises the result arrived at. He assumes that this result is the expression of a law, according to which any portion of matter reacts towards any other, and he formulates this great principle:

"Any two bodies attract each other with a force which is proportional to the product of their masses and in *inverse* ratio to the square of the distance which separates them."

"The general principle of gravitation," says Duhem, "was thus found; and this discovery was made without the application of any hypothesis, and through the inductive method devised by Newton."

I can understand that the laborious observations of the planets carried out by Tycho Brahe might be imagined to have been carried out without hypothesis, although even then—for instance, in the instruments used—there must have been employed constructive ideas. But Kepler's three laws were, after all, surely a construction of rare genius, and his result a model of mathematics. And then, finally, Newton's share in the work. Can this be said to have been done without the aid of hypothesis, and was it not really a striking instance of theoretical construction with the precise purpose

of explaining the movements observed? To express arithmetically the forces involved in terms of the mass of the heavenly bodies was at least hardly a self-evident proceeding. The fact that a sphere is proportional to the third power of its radius can hardly be said to lead, without further hypothesis, to the conclusion that the active forces are a function of the mass of the heavenly bodies. We know that Galileo rejected the idea that tidal movement could be due to action of the moon, owing to his fear of "hidden qualities." Duhem himself shows, in an interesting historical survey of the origin of the general theory of gravitation, that centuries passed before men ventured upon the hypothesis that matter, the mass of a thing, could exert attraction, and that even through great distances. In Bacon's day it was supposed to be the light of the moon, or rather the warmth of its light, which produced the ebb and flow of the tide; even Kepler himself believed that the "virtus movens" of the sun was, if not light itself, at any-rate a "virtus" utilising light as its carrier, and he believed he could from his own laws conclude that the action of this "virtus movens" stood in inverse proportion to the distance from the sun, not to the square of the distance.[1]

[1] For an account of these complicated but interesting errors, see Duhem, *loc. cit.*, pp. 404 ff.

It seems hardly likely that Newton, in establishing the formula $\frac{M_1 . M_2}{r^2}$ should have been unaware that he was making a great and bold assumption. It seems far more reasonable to explain his disclaimer with regard to the making of hypotheses as meaning that he did not intend to venture upon any explanation of the great riddle of what a force is, and how it can act at a distance; a problem which was hardly brought under discussion until a far later time, when the whole question of the relations between mass and energy was subjected to analysis and experimental investigation.

There is here an important general point to be noted. From the general law of gravitation it immediately follows that in the solar system as it actually exists we have not merely to deal with one sun and one planet in their reciprocal action. There must be reciprocal influence between all heavenly bodies. No human being will ever be able to calculate the result of all these interacting influences. Newton's law is therefore arrived at by an excessive simplification of the problem; by the arbitrary establishment of a theoretical system which cannot be found anywhere in nature, presenting two bodies, and two only, isolated from all other influences. This

method applies in principle to all physical investigation. It is therefore impossible to say that physics—or chemistry—investigates nature as it is, while biology in the same way only considers elements isolated from an equilibrium which analysis necessarily destroys. This is common to all science, and an acknowledgment of the fact is fundamental to the understanding of the method of every single science.

Another highly instructive example of the relation between hypothesis and observation is seen in the study of the gas laws. In this field it has long been maintained that hypotheses might be dispensed with altogether, that the results come, so to speak, of themselves. It has even been said that the results are "distilled off" from the facts.

This school has been characterised by W. Nernst[1] as the empirical, its method being to "endeavour, by suitable observations, to collect a wealth of material, expressed, if possible, in figures, with regard to the phenomena presumed to be inter-related, and thereafter, by a purely empirical juxtaposition of the results obtained, to arrive at the goal desired; thus, for instance, certain relations were discovered between the qualities of elements and their atomic weight." But the

[1] Walter Nernst, *Theoretische Chemie.*, 7 Aufl., p. 2. 1913.

words "suitable observations" and "the goal desired," themselves show that this method of work must be based on a *previous construction*, that is, on the establishment of a "system," an "experimental arrangement," which, as a matter of fact, in many cases consisted of a cylinder with its piston, an analogy suggested by the working steam-engine.

But, besides the empirical school, there is another altogether different which Nernst calls the theoretical. It "leads, hand in hand, with accurate conceptions as to the nature of certain phenomena and through a purely speculative process, to a new hypothesis, as to the correctness of which subsequent experiment must decide. Thus was discovered, by *kinetic* considerations of the chemical combination and dissociation of substances which react on one another, the law of chemical mass-influence."

Now it is in many respects highly interesting, from the point of view of biology, to consider these methods of theoretical chemistry. We can here, of course, merely pick out one or two points.

As in biology, so also here we find two camps with restricted points of view, each advocating its own method, or at any rate laying the chief stress on one side of the

complete scientific method established by Galileo. Each of these schools sets up its own distinct system. The empirical school works with the material in mass, ignoring any preconceived theory as to its molecular composition. It was in this way that Boyle and Gay Lussac discovered the laws which bear their names. The kinetic school, on the other hand, starts out from the conception of molecules, and seeks thence to arrive at the same results as were attained by the former system. It has rightly been considered one of the most interesting results of scientific research that it should have proved possible to attain the same results by two entirely different systems, one of which (the molecular system) could, by Boltzmann's idea of the average molecular velocities, be transformed into the other. Our knowledge of gases can thus be considered as concerned with the average qualities of the innumerable millions of molecules present in the gas.

Of the greatest importance to biology is the series of ideas emanating from the law of chemical mass-influence established by Guldberg and Waage, and the results of its application. For it has introduced into chemistry a new world of ideas. Formerly, a chemical reaction was regarded as the transformation of one body

into another or others, in accordance with the formula $A = B$; now, on the other hand, the view of such a reaction is, that in all cases, albeit in widely different degree, the substance A is also formed from B, and the entire process is thus expressed as $A \rightleftarrows B$.

This formula not merely indicates that there is a formation in both directions, once and for all, but that there is constantly, even where analysis cannot detect it, a certain amount of both kinds of formation taking place. The quantity of the products formed, therefore, will not appear as permanent amounts, but as states of equilibrium between the products which are constantly being reformed from both directions. How much is formed, depends on the conditions prevailing at the time, the strength of concentration (mass) in which the various substances are present.

It would be hard to find an example better suited to show how thought, in evident opposition to everyday experience, can revolutionise scientific ideas, and how this new idea opens up an altogether new possibility of comparison between living and lifeless objects. For the idea of equilibrium has always been fundamental to the idea of life.

Our third and last series of examples may be taken from the history of the atomic theory.

Since Dalton, at the commencement of the nineteenth century, revived the atomic theories of the Greeks, the structure and form of matter have become questions of ever-increasing interest and importance in the exact natural sciences. These two elements, *inter alia*, entered into Democritus' ideas of atoms. There was the ultimate indivisible form of matter (atoms), and there were innumerable kinds. For the study of atoms, theoretical models were constructed of a size and efficiency presumed sufficient to explain the phenomena observed.

"It is the relations which are seen by the mind's eye to be the logical consequences of exact measurement which are for the most part dependable," says Millikan.[1]

With these concepts, chemistry and physics (after Dalton) made a fresh start. Having worked out the relationships of atoms and molecules to one another, chemistry, both organic and inorganic, arrived at one of the greatest results in science, namely, the explanation of the composition of the great majority of substances in terms of the atoms and molecules, present in each, in characteristic numbers, weight and spatial arrangement.

Thus was achieved what some hundred

[1] Robert Andrew Millikan, *The Electron*. The University of Chicago Press, 1917.

years earlier, in Kant's time, would have been regarded as unthinkable, even by the boldest imagination, namely, a mathematical chemistry, and a form-system of chemical substances, based on the spatial arrangement of the atoms in the molecules.

The next step, which to my mind appears no less important in its general or philosophical aspect, was taken when Pasteur made his famous discovery, that dextrorotating and lævorotating tartaric acid — two substances then believed to be of like molecular composition—had each their own characteristic form of crystal, the two being alike in all respects save that one was a mirror-image of the other. If we except the previously established relationship between chemical (formula) and molecular weight, we have here, as far as I know, the first indication of any connection between form and function (the deflection of light) in non-living matter. Pasteur's discovery was extended by Le Bel's and van't Hoff's theory, that the difference between dextroactive and lævoactive compounds was due to "isomery" in the molecular structure of the substances. We find, for instance, an interesting treatment of this in Nernst's text-book above referred to (p. 319), from which it will be seen that he deduces *the chemical qualities from the distances*

between the relative position of the atoms in the molecule.

Another interesting relation is found in the work of Victor Henri,[1] who shows that there is a definite relation between the structure of the molecules and their spectra, and this in such a manner that the various radicals, of which the highly complicated organic molecules are composed, each bring about certain definite modifications in the spectrum, so that the spectra even of bodies not previously investigated could be foretold.

With regard to all these investigations it may be said that they assume, with Democritus, the indivisibility of the atom, or, at any rate, that they had no need of any other premise than that the atoms had a weight constant for each separate kind. It was just this constancy of weight which constituted the particular value of the method of using molecules and atoms for the explanation of the phenomena of analysis and synthesis. In the field of chemistry and physics, for this reason, the most energetic opposition was offered to the suggestion that atoms were only to be regarded as mental models for explaining the phenomena. A great mass of splendid experiments appeared most

[1] Victor Henri, *Études de Photochimie.* Gauthier-Villars, Paris, 1919.

decisively to support the view that the atoms and molecules were definite objective units and not only images for thought (cf. Hegel).

One thing is here overlooked, namely, the fact that even the idea of *size itself* conveys a suggestion as to form and surface, and is thereby already the idea of a model. But this is no argument against the *existence* of atoms, and it leads to the consequence that the assumption of a certain size for atoms does not involve as a logical necessity their indivisibility, nor does it follow that matter *must always appear in the same form and size.*

We cannot here disregard the thought that atoms and molecules correspond to the structure of matter *under certain conditions*; in other words, that there may be both smaller—more simply constituted—and more complicated *units*, and, further, that in the course of a long series of investigations, it may be *very useful to work and reckon with quite other units of matter than atoms and molecules.*

These ideas, which found their chief support in the pioneer work of Sir J. J. Thomson, which resulted in the discovery of small "corpuscles" many hundred times smaller than the smallest atom, the hydrogen atom, led, as we know, to an entirely new chapter in the history of science. It will suffice here to quote

the following enthusiastic and striking words of one of the pioneers of the new school of research.[1] "And now this twentieth century, though not sixteen years old, has already attempted to take a still bigger and more significant step. By superposing upon the molecular and the atomic world of the nineteenth century a third electronic world, it has sought to reduce the number of primordial elements to not more than two, namely, positive and negative electrical charges. Along with this effort has come the present period of most extraordinary development and fertility — a period in which new view-points and indeed new phenomena follow one another so rapidly across the stage of physics that the actors themselves scarcely know what is happening."

In the course of this research we find, instead of the old atoms, new and far more complicated corpuscular models set up, which, although perhaps as yet only the first ingenious attempts, certainly approximate to the truth. More than this, it has been shown how the unalterable basic elements of the "old" chemistry have become a system of two elements only, which in differing quantitative proportions build them all up into different structures.

[1] Millikan, *loc. cit.*

Here also we find the first striking attempts to establish a connection between structure and chemical properties, as, for instance, in the arrangement of the elements in the periodical system, and between structure and physical qualities, for instance (the spectra of Moseley). Indeed, even the idea of matter itself is in danger of being replaced by that of "electric charge." In other words, we have here a complete parallel to the statement about living beings quoted from Roux (*supra*, p. 54) to the effect "that they can be defined only *functionally*; that is to say, through characterisation of their activities."

It is of the greatest interest now to see that during the greater part of the past century there was strong opposition against this developing school of research. It was the actual idea of constructing models—machines for thought—that gave such offence. It is the empirical school's fear of the errors of the idealists which here affects the question. Thus we find specially active opponents in the field of thermodynamics, *e.g.*, Duhem, Mach and Ostwald. It is with evident reluctance that Duhem in the following words (*loc. cit.*, p. 159) concedes to the new school a certain justification: "Strong minds, those who have no need of concrete images in order to com-

prehend an abstract idea, should, after all, not refuse to synthetic yet weaker spirits, who find it difficult to grasp that which has neither form nor colour, the right of depicting, in imagination, the objects presented by theoretical physics. The best mode of furthering the development of science is to allow every intellectual form to develop according to its own laws, and produce its own type, that is to say, letting the stronger spirits feed on abstract ideas and general principles, while the others seek their spiritual nourishment in visible and tangible things." He quotes, in this connection, an interesting dictum of Helmholtz: "English physicists like Lord Kelvin, formulating his theory of atomic vortices, or Maxwell, imagining the hypothesis of a system of cells whose content is vivified by a rotary movement —the basis of his attempted mechanical theory of electro-magnetism—have plainly found more satisfaction in such explanations than if they had contented themselves with a purely general statement of facts and laws in terms of the system of differential equations commonly used in physics. I confess for my own part that I still feel attracted by this last type of idea, and have more confidence therein than in any other. But I should not wish to raise any objection on principle against a method

which has been followed by such distinguished authorities in physics."

. . . . . .

From the foregoing, as, indeed, from observation of any branch of exact science, it is apparent that in all investigations the fundamental logical elements of analysis and synthesis are both present. The analytical characters "are parts of an idea I already have in reality (which I already consider as embracing them); the synthetic characters, on the other hand, are parts of an idea which I still regard merely as a possibility (*i.e.* which will first come into existence by synthesis, the combination of several parts). The former are conceptions of reason, the latter *may* become conceptions of experience."[1]

Even in the two most extreme opposites, "empirical induction" and "mathematical deduction," the presence of both elements can be shown. Inductive conclusions cannot be drawn unless we have first set up some working hypothesis as a pure construction. The validity of this hypothesis is still a problem, which we endeavour to solve by testing the hypothesis against the facts, which we regard as certain and reliable. If, then, it is found that all the instances we have tested, or can

[1] Kant, *Logik*, p. 9.

think of testing, harmonise with the hypothesis advanced, we conclude that other instances also —nay, all possible instances—must likewise agree with it.[1] Mathematical deduction appears in reality to have arisen out of the purely logical side of intellectual activity, but in its creation or application to new instances the investigator will always have to make use both of the analytical and of the synthetic method; both of experience and of intellectual construction. In different series of investigations, however, we may distinguish between the relative scope of the two methods; in one series analysis, in another synthesis, will bulk more largely. And we often find—a very important fact—that such differences arise solely from the fact that the element of apparently minor importance has played a greater part in previous investigations in the same field. In other words, it is because we are considering too narrow a section of the science, that we imagine work in the one direction to have produced scientific results without any help from the other.

This dualism in scientific method has been a cause of considerable offence to dogmatic philosophers. And what lies behind this is, that they see therein a conflict with their own one-sided empiricism or idealism. Conse-

[1] Kant, *Kritik der reinen Vernunft*, p. 437.

quently, we find some writers endeavouring to eliminate the rational side, others the material side of human understanding. This dogmatic mode of thought entirely disregards, first of all the fact that science itself shows us how the two must be combined, and that all those who regard science as the sole basis for settling the question must regard the testimony of science as something not to be overlooked. But, further, it must be pointed out that the assumption of two kinds of elements in our understanding is quite a different thing from assuming the existence of two kinds of ideas materialised as "things." By way of illustration, which may perhaps serve to make the matter clearer, we may take the following. Kant called his method the "discursive method." A discursive movement means a movement first in one direction, then in the other; a discursive understanding will then be an understanding arrived at by movement in two directions, the analytical and the synthetic. Kant, in his criticism of human reason, makes various comparisons between his critical analysis of understanding and chemical analysis. But such comparison could not in his day attain anything like the same degree of clarity as now, since the only chemistry then known was the so-called static chemistry. The discursive

combination of the elements of understanding can in reality be far better understood by a comparison with the modern conception of the dynamics of reversible chemical combination, *i.e.*, according to the formula already mentioned above $A \rightleftarrows B$.

This formula expresses a state of equilibrium between two processes constantly taking place in both directions. Similarly, the scientific method may, according to the discursive conception, be regarded not only as a movement made once in each separate direction, but as a constantly alternating movement in both directions, finally arriving at a state of equilibrium which is yet not absolutely permanent, but depends only upon the analyses and syntheses which have taken place in each particular case. A continuation of these processes will also increase the understanding acquired; at one time it may be better furthered by the one, at another time by the other, kind of activity. Regarding understanding, then, in the light of this illustration, we find that understanding is itself after all a unity (an equilibrium), albeit arrived at by two processes which appear opposites to the *mind.*

It is immediately obvious from this that it must be impossible to establish any rule for what should be first and what should be last in

an investigation. Experience also shows us that investigations may commence from either the one or the other side ; the theory of understanding cannot set up any definite method which must always be followed in every separate case. But if we consider the task of science as an endless one, we can still regard the two movements of the understanding as parts of an ideal process, and we can, from their logical content, show them to be the necessary steps or stages in this ideal method of procedure. This, however, can only be done subject to the express condition that the stages must not be regarded as *simple* processes. For, in reality, there is a constant movement in both directions, and it is therefore only as a mental abstraction that one can set up certain groupings, by which research in any case will never feel bound. Nevertheless, such a scheme has after all a certain importance, and in all practical science, therefore, it is customary to lay down a certain plan for the work to be done, such a plan being of value, on the one hand, in making clear what operations are embraced by the given investigation, and, on the other, in seeing that none of the necessary elements are omitted. I distinguish between four such groups or stages :

1. Formulating and defining the problem.
2. Empirical investigation.
3. Formulation of the result arrived at.
4. Critical judgment of the accuracy and reliability of the result.

With regard to these groups I have a few general observations to make, chiefly with a view to facilitating the understanding of what follows.

1. *Formulating and defining the problem.*—The problem is chosen by the scientific imagination, which immediately perceives, or after careful consideration decides upon, a task to be done. A necessary condition for the choice to prove fertile is that the investigator should see a clear possibility of apprehending the object under consideration as a compound of simpler elements. "Object is that, in the idea of which the manifold is united in a given view," says Kant. For it is only thus that the object can be made the starting-point of an investigation. In physics, the thing subjected to investigation is generally called a "system." By this is understood any object considered in isolation from the rest of the universe. It may be a gas in the cylinder of a steam-engine, a liquid in a glass dish, a solar system, a group of molecules, or a single atom. The limitation

depends solely upon the aim of the investigator in that particular investigation. Kant defines a system as "the unity in manifold understanding when subordinated to an idea." A thing, then, can only be an object or a system for investigation, when there is some idea or aim determined on for such investigation.

From this it follows that the choice must be preceded by consideration, by a reflective process, since the choice can only be based on the supposition or hypothesis that the object can be regarded as suitable, *i.e.*, calculated to further the advance of knowledge. The idea of this, again, can only be arrived at by building on the "consciousness of relation between the given ideas to our various sources of knowledge." This reflective process in itself gives nothing new to science in the way of experience; it merely opens the road to procuring such experience.

The reflective process must be based upon a prior more or less definite recognition of the simpler elements within the system to be investigated, *e.g.*, the volume-elements of a gas in a cylinder. These elements do not integrate the entire object as such, but only in respect of the precise investigation in hand. Every object can be resolved into an endless number

of elements, and in an endless number of different ways. Every definite human investigation, on the other hand, is a finite and limited task. The complete appreciation of the object can therefore only be set as the ultimate aim of an indefinitely advancing science. "What the thing in itself is I do not know; but I do not need to know it, since it never occurs in experience," is Kant's famous dictum. "The thing in itself," says Høffding,[1] "stands as the great X, which cannot be scientificially defined; yet it is a definition of this which has been attempted in all religions and all speculative systems." In definite scientific investigation there can be a question only of one formulation of a problem leading with a definite purpose to a clearly defined goal.

2. *Empirical investigation* in natural science proceeds, as a rule, by so-called induction, that is to say, the plan being decided on and the objects and means of investigation (apparatus) chosen, a great number of measurements are made, which can always be ultimately reduced to very simple operations (measurements of length). But the making of such measurements always necessitates apparatus and methods of work based upon the work and

[1] Høffding, *Totalitet som Kategori*, p. 58.

invention, constructions and ideas of numerous previous investigators.

Induction leads to verification either by adding more and more facts to the hypothetically assumed law, or by producing, experimentally, the hypothetically assumed possibilities which agree with the law.

3. *Formulation of the result arrived at.*

By means of the measurements or observations carried out, we procure material for establishing the result. This, then, appears as a construction, *as a new system, a new totality with a different content from that which formed the starting-point.* It may in its turn again form the starting-point for new problems of another sort than those which formed the basis of the investigation whence it arose, and may in such case be resolved into other elements than those on which the former investigation was built.

4. *Critical judgment of the accuracy and reliability of the result.*

The criticism of this is partly given by the results arrived at through the deduction from the law or regularity found, and it further makes use of self-criticism, the psychological analysis to which the investigator subjects his own work, as in the so-called estimates of probable error made in physical investigations,

where it is a question of the degree of accuracy with which the work has been done. Naturally, then, the investigator will be disposed to ascribe the greatest value to his results if his calculations of probable error, his self-criticism, give him the impression that his work can be relied on. The more extensive and the more accurate the personal work involved in an investigation, the greater will be the satisfaction, and sense of understanding nature, derived from the result. From this it appears that the constructive side of research is that which yields the greatest satisfaction. In this connection it is interesting to call to mind the words of Kant, that it is a great advantage "to connect the study of nature in its mechanism closely with that which we can subject to our own observation or experiment, in such a way as if we were ourselves producing it in the same manner as nature, or at least by like laws; for we are only able to grasp entirely that which we can ourselves produce or bring about in accordance with certain conceptions."

It is doubtless for this reason that the sciences where personal activity is greatest, such as mathematics and mathematico-physical studies, appear to give the greatest certainty. Not because these sciences in reality—from the point of view of the theory of under-

standing—are based on other elements than those of any other scientific activity; it is a psychological reason, nothing else, which lies behind our attitude to this question. For in all comparison of physics with mathematics, it is of the greatest importance for the naturalist to call to mind anew what I have already quoted from Høffding, namely, that there is only an analogy between mathematical ideas and physical facts.

On the basis of the points of view here expressed, I will now, in the last chapter, endeavour to make a comparison between biology and the exact sciences.

V

THE UNITY OF SCIENCE

THE idea of "life" conveys to all of us a deep sense of something extremely complicated. Complicated, not only in structure, but also in function. The idea of movement is, to many minds, connected with those of activity and consciousness, and it is perhaps not least these ideas which from the earliest times have led mankind to divide natural objects into two kinds, namely, dead things and living beings.

In science, however, and more particularly since the theory of evolution has made its influence felt, there has been an increasing tendency towards the belief that the great distinction drawn by us between dead and living objects is due merely to the imperfection of our knowledge, and not to any difference in principle between the things themselves. The constant increase in the number of organic syntheses, the exact demonstration of the law of conservation of energy in living and dead

objects, and, finally, the teaching of geology, that the earth was once a glowing mass, have contributed largely to this belief. Innumerable attempts have been made in the course of the centuries to ascertain whether living beings may not spontaneously arise from dead material, and it has been frequently asserted, ever since the time of Lucretius, that this actually took place. But a critical examination of all these assertions has hitherto led to a negative result, though this, of course, cannot be said to have disposed finally of the question itself.

It will perhaps be best to postpone the observations I have to make on this head until we have first considered another, possibly no less pressing question, to wit, whether there is, or need be, or should be, any difference in the methods and mental attitude of science in the investigation of living beings as compared with the study of inanimate objects. A scientist's standpoint with regard to this question will affect every one of his biological observations.

Obviously, a comparison between two different sciences cannot embrace more than the most general results of the sciences in question. It is obvious, also, that a comparison of methods and their applications must keep closely to the method of thought and the mental attitude shown in the two cases. This,

of course, necessitates abstraction from everything of a special character, and a search for the common features which occur, under different forms and names, in the two.

We may derive considerable assistance here from a consideration of the questions so clearly formulated, albeit not solved, by Kant (see p. 28).

1. Whether physics and chemistry will ever be able to set up any system of morphology corresponding to that of biology.

2. Whether human consideration of living beings can ever give us the same expectation as we have in mechanics, of being able ourselves to produce the phenomena.

3. Whether it will ever be within human power to regard the problem of purposiveness from the same point of view in the two groups.

I will take these three questions separately.

Morphological Science

As we have seen in the two previous chapters, a criticism of the standpoints of the different sciences leads to the conclusion that no science, whether biology, physics or chemistry, can dispense with the establishing of a certain form or structure in the object. In biology this is called *organisation*, in physics and chemistry,

structure. In all three sciences it has further been found that the assumption of a structure did not in itself constitute a solution of the problem. Only by assuming an intimate connection between form on the one hand and qualities on the other was there a possibility given for adequate research and fuller understanding. In biology, this connection is stated as being between organisation and function; in chemistry, between molecular structure, molecular weight and chemical "affinity"; in physics, between the corpuscular model and the forces involved (electricity).

Can we now under all the different names used in the different sciences, recognise the same conceptions and ideas? To do this we must at least be able to explain psychologically —according to Hume's method—what is implied in the difficulties which are evidently present and hinder thought from not only feeling certain similarities, but perceiving full agreement between the two series.

(*a*) For most biologists the greatest difficulty arises from the idea originally present in our minds, that living organisation is something far more complicated than the structure established by physics or chemistry. This sense of greater complexity should presumably be reduced on regarding the thought-models

of chemistry and physics. The hypothetical edifices erected by all three sciences seem long since to have approached, in point of complexity, the limits of human conception.

Sir E. Rutherford is undoubtedly right when he says that "the more the subject is examined the more complex must we suppose the constitution of matter in order to explain the remarkable effects observed." *The complexity of an object depends not on itself, but of the degree to which it is investigated, and the questions we ourselves raise in investigating it.*

But why then have we raised so many more questions in our study of living beings? The answer must be that we are so much better acquainted with the morphology of living beings than with that of inanimate things. It is only during the last generations that physics and chemistry have sought to establish a morphology, whereas zoology and botany, anatomy and physiology have been at work on this for over two thousand years.

Biology is in this respect, as in others, the oldest of all the sciences. The study of its history clearly shows that the questions we nowadays raise, and the many ideas we now possess, would never have been possible without the toilsome and extensive work which has

been carried out in the field of biology during those two thousand years.

(*b*) But apart from the manifoldness of our knowledge of biology, the idea of complexity contains yet another important element, and one which we often meet. We find, for instance, the demand advanced by Jacques Loeb (*loc. cit.*) for, "as the ultimate aim of the physical sciences, the visualization of all phenomena in terms of groupings and displacements of ultimate particles; and since there is no discontinuity between the matter constituting the living and non-living world, the goal of biology can be expressed in the same way." It is not difficult to perceive that we have here the attitude of Darwinian dogmatism, the aim of which is to establish a *continuity* "from below upwards" — from the ultimate particles of matter, up to man. Loeb finds it inconsistent with this aim to set up any special biological systems; his ideal at least is to reduce them to molecular systems.

I have in Section III. pointed out the entire futility of asking science to give a complete description of the course of evolution, a task as impossible as it would be for physics to give a description of every single wave, or for Lyell to have given other than the most general description of the geological alterations of the globe.

And, indeed, the simplest consideration will show us that it is not a characteristic feature of biology alone to set up systems which cannot *as such* be resolved into atoms or molecules. The Newtonian investigation of the solar system dealt, for instance, with highly complex objects, which by no means could or should—at any rate not *in Newton's investigation*—be brought into any sort of relation with the ideas of atoms or molecules. Newton's method of dealing with the solar system consisted in a drastic simplification of that system, an abstraction from all possible points of view save one. When the model thus constructed was complete, the investigation could commence, and its highly scientific character lay, not in the place occupied by the model in an evolutionary series of matter, but in the lucidity, acuteness and imaginative power of the intellectual treatment.

Retaining, however, the idea of an evolutionary series, it is clear that science must always take this from every possible quarter. It is no more scientific to study the Protozoa than it is to study Vertebrates. When the science of physics is no longer content, as now, within the main working downward towards the minutest particles of matter, but begins to work upward as well, or constructively, towards

more complex systems, then physics also will have to establish, and that to an increasing degree, systems different altogether from molecular systems, — systems, in fact, which (at any rate for the purpose of the investigation concerned) disregard molecules altogether.

An instance of this is seen in recent researches on the subject of colloids. In such researches, it has been found that suspensions contain particles which must be regarded as great aggregations of molecules. These particles behave in regard to osmotic pressure (*e.g.* in the determination of freezing-points) as units, just as in some substances molecules, and in other atoms, do. And it is the form which the matter takes in each of these cases that determines the unit to be selected for the system in order that—to mention only one reason—the mathematical formulæ for osmotic pressure may hold good. In other respects, as, for instance, in respect of surface action, colloidal particles exhibit quite other qualities than do molecules proper.

In the same way, biology must set up a different system for each particular investigation. For the nature of the system must depend upon the material construction which the investigator *himself*, quite arbitrarily, but hypothetically, takes in each case as the starting-

point of his investigations. The question as to whether he has chosen the right system will be answered by the nature of the results obtained. Many investigators have in the course of time struggled with vague ideas as to this. The most striking case is Goethe.

Goethe's entire view of nature was incompatible with the fact that white light is dissolved by Newton's prisms into the varied colours of the spectra. He has been rightly criticised for his attacks on the physico-mathematical investigations of light. And yet after all there was in his vague ideas something of the insight of genius; instinctively he felt that here was a problem that could not by any means be resolved by Newtonian physics, but must be *kept as it was.* The explanation lies in his remark (in the work on colours) that *colour is something relative to the eye.* This I take to mean that there is a particular problem connected with the biological unit, the eye, with its construction or organisation. Physiological research has since shown that the eye is incapable of analysing white light (as do Newton's prisms); the eye gets the same perception of a single wave-length as of two whose average length corresponds to that of the first; moreover, certain pairs of colours produce the impression of white light.

These things cannot be understood without assuming the existence of a peculiar mechanism or organisation acting in such a way as to produce the results observed, and such a mechanism cannot as such be reduced to a system of atoms or molecules. Later physiological research has been occupied even to this day with the task of discovering this mechanism. The eye is, of course, composed of atoms and molecules, but the relation of these to one another, and their position relative to the thing we call an eye, must be regarded, in principle, as ranking equally with the model of an atom. The eye is a unit, just as is the model of an atom.

In no biological system does the problem of higher and lower systems count for so much as in the nervous system. It is highly interesting to note that David Hume himself was already able to form some conception of the fundamental idea that the nervous system is built up of higher and lower centres. With regard to the human power of combining many details into units, he says: "It is more conformable to the ordinary wisdom of nature to secure so necessary an act of the mind, by some instinct or mechanical tendency, which may be infallible in its operations, may discover itself at the first appearance of life and thought, and may be independent of all laboured deductions of the

understanding. As Nature has taught us the use of our limbs, without giving us the knowledge of the muscles and nerves by which they are actuated, so has she implanted in us an instinct, which carries forward the thought in a corresponding course to that which she has established among external objects; though we are ignorant of those powers and forces on which this regular course and the succession of objects totally depends." It is this same instinct which leads a practical man, whether scientist, man of business or workman, to oppose any over-close analysis of problems where his judgment of the situation in perspective shows him that he can better work with the complex units as they are; it is this practical view also which is expressed in the old proverb as to those who "cannot see the wood for the trees."

Duhem (*loc. cit.*) has endeavoured to establish a difference of principle between physiology and physics, in that it is only in the former, never in the latter, that there can be any possibility of an *experimentum crucis*; that means, in this connection, a single experiment which finally and conclusively determines the solution of an entire problem. Physics may measure and measure *ad infinitum*, and yet never attain to more than approximate results; whereas, in his view,

physiology can arrive at a definite result. He quotes by way of example the fact that physiology can, say by cutting through the anterior roots of the spinal nerves, determine whether their function is motor or sensory. But is this because it is physiology and not physics? Is it not rather because the question concerned is not one to be dealt with by a purely empirical, but rather by a constructive method, one assuming an *organisation in the system*? As soon as the conception of the investigation contains any idea of organisation, any constructive idea, then a single experiment may suffice to show whether there is an actual logical relation between the constructional and the functional idea corresponding thereto. It is by this *originally biological method that the entire modern investigation of atoms and molecules is guided.* It seems to me, therefore, particularly interesting to find that a man like Helmholtz, who in his studies of the mechanism of the eye and ear, himself built very largely upon the basis of organisation in nature, should, as we have seen above, oppose the use of thought-model in physics. And this despite the fact that he had himself established the law, valid both for animate and inanimate objects, of the conservation of energy. This shows that the idea of an entirely parallel consideration of structure in living and dead things

has up to the most recent times been regarded as an impossibility both in practice and even in theory.

(*c*) But why should there be so many more mathematical symbols and calculations employed in physics? This, again, does not lie in any difference of principle between physiology and physics. We have now in biology an enormous number of examples of the most exact use of mathematical symbols. Thus Hopkins says (*loc. cit.*): "Truly the 'bio-chemists' had to share ignorance which was universal, and sometimes compelled by the urgency of some problems, had perforce to do their best in regions that were dark." "As for analytical work involving a separation of complex organic mixtures, the bio-chemist of the last century was in this ahead of the pure organic chemist, as the development of urinary analysis if considered alone will show." On the other hand, it is necessary to bear in mind that mathematics is, after all, only an implement, applicable to the very simplest abstractions. The slightest complication of the problem renders mathematics a wasteful tool in the service of research. The comprehensive power of eye and brain can in many cases deal with complex relations far more advantageously than can mathematical analysis. It has proved possible to show, for instance, by

means of extensive and elaborate mathematical calculations, that the attachment of the muscles of the arm to their bones exhibits marked agreement with mechanical principles. For *this* comparison mathematics was the most adequate and useful method, its employment, indeed, a direct necessity. But for a description of the simplest movement of the various muscles of a human arm—how they take place in space and time—mathematical calculation would be altogether superfluous.

It is by no means only in the treatment of biological problems that science makes use of other than mathematical methods. Thus the cinematograph is used for description of movements which would force even the ablest mathematician to give up all hope of treating them mathematically. But, as a matter of fact, the human eye is just such an apparatus in itself—at any rate, within certain limits of rapidity of movement.

Physics also make use, and that in the greatest measure, of the more complex systems, where mathematical treatment of their elements would be an impossibility. This applies, as we have seen in the last chapter, to thermodynamics, for instance, which works with unitary volume of gas (litre-atmospheres), and not with molecules. The study of molecules, so

soon as we start to consider not merely one or two of them, but many, can only progress by the introduction of statistical methods, *i.e.*, averages of the velocities, volumes, etc., of the single molecules. The drawing up of these averages is, however, in reality equivalent to the setting up of a new system, infinitely simpler, and thus able to serve as the starting-point for an extremely simplified mathematical treatment.

In biology, where research has proved the possibility of setting up a long series of higher and lower systems, we may say that there has been developed a special method for *system-analysis.* Take, for instance, the nervous system, and such series as the muscular mechanism, the single muscle, the separate muscle fibre, the elements of this fibre, and so on. This method will also at some future date be utilised in the physico-chemical sciences, when they come to work with "higher" units.

Physiological Science

We have seen in the foregoing that Kant, in setting up an ideal for the understanding of nature, declared that we know "only that which we can ourselves produce according to

our own ideas." When Galileo could formulate the law of the velocity of falling bodies beforehand, and show by experiment that nature followed his law, he had satisfied this demand. When a chemist can produce silver chloride by mixing sodium chloride with silver nitrate, he has formed clear ideas about it, and can produce the phenomenon at will.

In the literature of physiology, on the other hand, we often meet with the assertion that vital processes cannot be controlled and reproduced in the same way as physical and chemical processes; the term "vital process" being used particularly to designate that which eludes experimental imitation. When we come to the organism itself, we recall the famous dictum of Kant, that it would be unjustifiable for mankind even to hope for the advent of a Newton who could render understandable even the production of a blade of grass by mere mechanical laws.

An examination of the state of science in Kant's day does indeed give one the impression of a state of opposition in principle existing between biology and physical science. Since then, however, we have gained such a wealth of experience and systematised knowledge that the apparently insoluble problem can once more be discussed.

In the first place: what was it that Newton did produce? As we have already pointed out, it was the creation of a highly simplified model, arrived at by abstraction. It is never "the object in itself" that is mastered or produced by physics or chemistry, but only simplified aspects, or in other words, perhaps, highly restricted ideas of it.

When Newton established the law of gravitation he established a relation between the two concepts of mass and attraction. He did not give any explanation of, and did not indicate any mechanism for, the action of one mass on another.

When physics established the relations between heat and mechanical work, or between heat and electricity, these relations did not imply an explanation of the transformation of one into the other.

When biology establishes a relation between fluctuating variations and the normal curve of error, this relation is not to be understood as anything more than an analogy.

Why then should the method of Cuvier, the establishment of relations between living organisms and the environment, be in any sense less scientific than the methods of physics or chemistry? It happens sometimes in all three sciences that such relations are found to be

based on mistakes, or to require some sort of modification.

When a chemist "produces" silver chloride, we know now that he is only occasioning a rearrangement of elements already existing as such, although he may perhaps at some future date come *nearer* to producing these elements from others.

When Mendel, by his crossings of two different forms, produced a third form differing again in certain respects, his work here is in *principle* similar to that of the chemist.

It has been said very often, that physiological chemistry only studies the dead products of living beings, but of physics and chemistry also, it may be said that they have to analyse and disintegrate in thought the equilibria existing in nature before they can, by an extensive simplification, attain to any useful results. These sciences then never deal with the objects as such, but only with certain simplified ideas of them.

However far science may be able to progress in the direction of analysis and synthesis, it will, of course, always have to start out *from definite preliminary material*, and will accordingly be limited in respect of the grouping of its elements. In the ancient question as to the "production of life," therefore, there can never

be any suggestion of doing more than producing a certain equilibrium. It can hardly be doubted that scientific analysis of living organisms is at present very far indeed from any understanding of the vital equilibrium, and, consequently, still farther from its synthesis or production. When we consider what has been done during the last hundred years towards the understanding of this problem—it will suffice to mention Emil Fischer's great work on organic synthesis—it seems altogether unjustifiable to regard a solution of the problem as in *principle* an impossibility, and from the point of view of the critical philosophy, at any rate, there is nothing to prevent the acceptance of the problem as a justifiable object for scientific research. But it may be said that the problem, like that of Galton and that of the inheritance of acquired characters, must start out from definite ideas and definite material, and that it involves, like the others, the enormous difficulty of foreseeing the existence of a new form of life in all its infinite complexity and multitudinous relations.

A comparative view of the methods of biology and physics thus leads us to the conclusion that the two sciences make up *a connected whole*, and that each can therefore learn a great deal from the other. Thus out

of anatomy and physiology, there has arisen a fertile method which has precisely aimed at regarding the structure of organisms as a mechanical arrangement (cf. Descartes). The interesting application of chemistry and physics to biological studies during recent years has also served further to efface the boundary between biological and physico-chemical methods. That this has not been more rapidly accomplished is due perhaps to the fact that all technical work has for practical reasons been mostly concerned with materials (metals) very different from the colloidal compounds of which organisms consist. A considerable change seems now to have taken place in this respect. It is a striking fact that men like Pasteur, Von Helmholtz, Faraday, Guldberg, van't Hoff, Arrhenius, Emil Fischer, have in reality played as important a part in biology as in the "exact" sciences. Biology has itself become an exact science.

Clearly, such a standpoint cannot admit the existence of processes of a specially "vital" nature, nor of any of the "factors" invented by neovitalism. Similarly, in biochemistry it is impossible to maintain that there is any difference between living and dead molecules, if any unity of view is to be attained. All chemical investigations have now to start from such

units as atoms, molecules or colloidal particles. To chemistry, therefore, no system can ever be thought of in any other way than as consisting of highly complex equilibria—that is to say, of complexes which have molecules as their simpler elements. More than this cannot be ascribed to systems in living beings *from a chemical point of view.*

Hopkins gives (*loc. cit.*) a very clear exposition of this view: "The highly complex substances which form the most obvious part of the material of the living cell are relatively stable. Their special characters, and in special the colloidal condition in which they exist, determine, of course, many of the most fundamental characteristics of the cell: its definite yet mobile structure, its mechanical qualities, including the contractility of the protoplasm, and those other colloidal characters which the modern physical chemist is studying so closely. For the dynamical chemical events which happen within the cell, these colloidal complexes yield a special milieu, providing, as it were, special apparatus and an organised laboratory. But in the cell itself, I believe, simple molecules undergo reactions of the kind we have been considering. These reactions being catalysed by colloidal enzymes, do not occur in a strictly homogeneous medium, but they occur, I would argue, in the

aqueous fluids of the cell under just such conditions of solution as obtain when they progress under the influence of enzymes *in vitro*."

The Teleological View of Nature

I have endeavoured to show in the foregoing, that to regard organisms from the point of view of the accidental cannot lead to any understanding of them. Such understanding demands, on the contrary, "an idea of a whole, upon which the composition and activity of the separate parts depend." In several passages in Kant's *Kritik of Judgment* we find the suggestion that in endeavouring to understand this idea of the whole, there is a sense of encountering a kind of "architectonic intelligence" in nature. Every one who has endeavoured to penetrate into biological problems will have experienced something of the kind—it is perhaps most strongly felt in embryological and evolutionary studies (cf. Bergson's attitude in his *L'évolution créatrice*), since we are here almost forced to recognise the existence of an impulse towards a goal or end. The idea of function in physiology gives us also the same impression.

Now it is undoubtedly this inner sense or feeling which causes the great divergence

between the attitude of the various schools of biology towards the philosophy of biology itself.

Empiricism (or materialism), taking warning from the discouraging results of vitalistic dogmatism (*e.g.* Hegel) and reacting from this one-sided and unrestricted activity of human thought, has attached chief importance to what Kepler called *veræ causæ*, and it stands in such fear of architectonic views generally (Mach, Duhem, Loeb) that it declines to make use of the constructive method at all. We are led inevitably to suspect that there must be a psychological cause for this. Scientific work has, as we know, become so complicated of late, requiring such a highly specialised technique, that *practice* in mechanical operations becomes of increasing importance. Now it is a psychological fact that all acquired ability becomes transferred from the conscious sphere to that of habit and routine. From the point of view of the physiology of the nervous system, one would say that the transference was from the hemispheres to the lower centres. No man whose mind is occupied with reflection upon his work can work so accurately and rapidly as one who has become so practised in the work that it has for him more of a mechanical character.

Now because empiricism tends to disregard the importance of pure intelligence in the establishment of scientific knowledge, we find among the empiricists numerous attempts to explain the origin not only of science and knowledge, but even of consciousness itself by imperceptible transitions from the lowest to the highest animal forms,—without regard to the fact that this is presupposing all the time the existence of the thing whose origin it is desired to prove, and thus moving in a circle. An interesting instance of this is seen in the following: Herbert Spencer put forward the interesting suggestion that living beings have developed their capacity for mental life in connection with their external conditions of life. He has regarded knowledge as a weapon in the struggle for life, as an "organon" which has been in use throughout the succession of generations, and has thus become of great importance in the struggle for existence. Høffding[1] observes in this connection that similar views have been advanced of recent years by Avenarius and Mach in Germany, and by James in America, in the so-called pragmatic philosophy. They point out that science contains merely the principles and primary conditions necessary for

[1] "The Influence of the Conception of Evolution on Modern Philosophy," *Darwin and Modern Science*. Cambridge, 1909.

the simplest and clearest orientation in the world of experience. All assumptions not applicable in experience and in practical life will in course of time disappear.

"In these views a striking and important application is made of the idea of the struggle for existence to the development of human thought. Thought must, like all other things in the world, struggle for life. But this whole consideration belongs to psychology, not to the theory of knowledge (epistemology), which is concerned only with the validity of knowledge, not with its historical origin. Every hypothesis to explain the origin of knowledge must submit to cross-examination by the theory of knowledge, because it works with the fundamental forms and principles of human thought. We cannot go farther back than these forms and principles which it is the aim of epistemology to ascertain, and for which no further reason can be given."

Thus empiricism also naturally operates with human thought. This is especially apparent when it becomes a question of formulating problems and of discussing results. But the empiricists' fear of trusting in any degree to the application of the constructive powers of the mind has often proved an obstacle to scientific progress. Witness, in biology, for

instance, their dislike of the idea of organisation, and in physics their opposition to the atomic theory or (later) their insistence on regarding the atoms as absolute and indivisible units, instead of as fertile models for the explanation of the problems of experience.

Vitalism, in contrast to empiricism, views the "architectonic intelligence" which it claims to encounter in living beings as something *objective.* It believes in what Kant called "objective teleology," and struggles vainly to find expression for its vague ideas of what this "something" is; whether it should be called a "factor," a "psyche," or a "vital force." In Socrates it is the divinity, in Aristotle the soul, in neovitalism a factor negatively defined, yet regarded as real.[1]

The enormous hindrance presented by this dogmatism, throughout thousands of years, to the progress of humanity and science is especially noticeable in the systems of the Middle Ages. Its essential outcome is to check man's investigation of objects in nature by offering him the less fatiguing flight of fancy as a means to knowledge. We have seen, for instance, how Hegel rested content with the idea of evolution as realised only in the world

[1] See, for instance, Hans Driesch, *The Science and Philosophy of the Organism.* Black, London, 1908.

of thought and conception. In our day, the influence of this school is especially noticeable in the use of such a phrase as "vital process" to express vague ideas which do not even attempt to establish any energetical system, or any analysis of the complicated "biological processes" into simpler systems which might be amenable to treatment by the methods of physics, chemistry or mathematics.

In order to understand the attitude of vitalism when it claims to discover an architectonic intelligence in living beings, it is particularly useful to consider Hume's observations on the subject of identity.

"When I turn my reflection on *myself*, I never can perceive this self without some one or more perceptions; nor can I ever perceive anything but perceptions. 'Tis this composition of these, therefore, which forms the self."

"We can conceive a thinking being to have either many or few perceptions. Suppose the mind to be reduced below the life of an oyster. Suppose it to have only one perception, as of thirst or hunger. Consider it in that situation. Do you conceive anything but merely that perception? Have you any notion of *self* or *substance*? If not, the addition of other perceptions can never give you that notion."

"Identity depends on the relations of ideas; and these relations produce identity, by means of the easy transition they occasion."

According to Hume's criticism of the idea of the self, there cannot be any objective self that we encounter when we study living beings. What happens is either that we recognise an idea, a system, a plan of organisation, which we have *ourselves* derived from some original idea, or from our scientific knowledge, or that we "conceive" a new *idea* of the architecture or construction in question.

In the study of embryology it is our previous knowledge of the organisation of the adult animals that gives us the strong conviction of the "prospective" importance of the young stages of the egg. The method of embryology (and of evolution) consists in an often repeated (discursive) consideration of the beginning and the (previously known) end of the changes taking place in time.

But in the development of modern physics and chemistry, the same thing is now happening to the physicists.

Regarded from this standpoint, it is no longer merely living beings, but the whole of nature which has become an architectonic structure, the richer and the more complex the more it is investigated by science. *It is*

science which has created our conception of this architecture, and its extension from the living into the inorganic world makes the whole structure not less, but still more wonderful.

From this point of view, the *word organisation is a biological expression for the same thing as is known to physics and chemistry as a system.* We have seen in the previous chapter that Kant defined a system as "a unity in manifold knowledge subordinated to an idea." This definition is equally applicable to our concepts of organisation and adaptation.

This view is supported by the fact that physiology has lately to an increasing degree made use of the word system in the same way as physics and chemistry.

A closer examination of the actual use in science of the two concepts, "organisation" and "system," will reveal to us the unity in our mind from which they both arise. The idea of an organisation as well as of a system can arise in two different ways, from a consideration either of the form or of the function of an object. If it arises from the form, we logically deduce a function; if it arises from the function, we must imagine a form. In both cases *it is the relation between form and function which establishes the organisation or the system, and it is only by this combination of the*

two conceptions that we have a starting-point for investigation. The linking together of morphology and physiology—of organisation and function—of structure and activity (force)—is the teleological method of thinking in science.

In biology, where form in most cases can be seen with the eye, this is usually the starting-point of the investigation, but in physics and chemistry the forms (the atoms) are invisible, and their existence is deduced from the phenomena, which are the consequences of the functions or activity of matter. I might refer in this connection to Millikan's statement quoted above, that he regards this logical deduction by "the mind's eye" as "the most dependable"; but we need hardly go as far as saying this.

This difference in the way of thinking has done much to conceal the unity of both sciences. For the difference is in reality not fundamental. We have seen, for instance (in Section II.), that Cuvier, using his wonderful theoretical conception of correlation, could from his knowledge of the functional activities of herbivorous or carnivorous animals draw logical conclusions as to their organisation. We have seen, too, that the experimental knowledge of two forms of crystals of two isomers could lead to the idea of different functions, corresponding

to each of the two forms (the dextro- and lævo-rotatory substances).

In both sciences it is only the combination of form and function that establishes scientific truth. Paul Bert[1] expresses this thought in the following way: "Even in cases which are concerned with details of the most instructive kind in anatomical research, or which apparently disclose the most simple mechanisms, it is best not to draw any conclusions from anatomy" (as to function) "before they have been sanctioned by experiment." This is the same standpoint that Galileo took when he rejected logic as an adequate method of research if unsupported by experiment. In the same way the logical assumption of atoms or molecules has no certainty if not confirmed by experimental evidence as to their functions.

In the history of science we meet different *names* for this relation between form and function. We have in biology entelechy, harmony, equilibrium. The two last words occur in physics as well, and it is interesting to remember the old saying of Leibnitz: "Existere nihil aliud esse quam harmonium esse." Looked at from the standpoint of the critical philosophy the saying must be interpreted in this way, that

[1] Paul Bert, *Leçons sur la physiologie comparée de la Respiration.* J. B. Baillière, Paris, 1870.

nothing can be an object for science outside its relations. The proper objects for the science of biology are the organs in their relations to other organs and to the organism as a whole, and the whole organism in its relation to environment,—the parts of an organism through their functions, the whole organism through its adaptations.

But why do these different names still persist, if they in reality refer to one and the same thing? It seems to me that the explanation of this fact must be found not in any difference in what the names represent, but in our own attitude towards the various scientific objects. The names represent, so to speak, stages in our ability to deal with scientific problems—not stages in the history of science in general, but stages in the history of each problem. There are accordingly—even at the present day—problems which can be treated most naturally in the way indicated by the more primitive concept, and other problems which can be approached better by the more "exact" method. Entelechy and harmony, for instance, give us a more primitive conception of a problem than does the concept of equilibrium. It seems, however, to me quite a mistake to believe that a research concerned with the concept of equilibrium should for this reason be assumed

to be of the highest scientific value. Such work may rightfully be said to have attained the *most scientific form*, but the beginnings of new lines in science may have a more primitive form and yet represent the greater scientific progress. I need only refer to the work of Darwin and Faraday, to the discoveries of X-rays and Radioactivity as compared with the numerous far more elaborate investigations which followed these great beginnings. It is also interesting to remember in this connection that Kant and his contemporaries found it unthinkable that chemistry ever should develop into a mathematical science. The history of science gives us the clearest evidence that physics and chemistry have had steps or stages of exactness in all their problems just as biology has had (cf. such a principle in modern chemistry as that of Le Chatelier). It seems therefore to be doubtful whether it would be of any value to science to try to eliminate the use of the old concepts in scientific problems and activities. But it is certainly of great importance to obtain a right understanding of the great principles involved in them.

Of great importance is the understanding of the relation between the two conceptions, organisation and mechanism. Hopkins says (*loc. cit.*): "Certain of the phases," of the cell,

"may be separated, mechanically or otherwise, as when we squeeze out the cell juices and find that chemical processes still go on in them; but 'life,' as we instinctively define it, is the property of the cell as a whole, because it depends upon the organisation of processes, upon the equilibrium displayed by the totality of the two existing phases." A mechanism is a part of a whole or an abstraction of one side or aspect of a whole. The word organisation implies the concept of an "infinite complexity," what Kant called "Das Ding an sich," and what Høffding called an infinite task, or an ideal for an infinite science. Organisation as such can never be an object of investigation; it is the fertile source for experience, to which all investigations must return again and again. It consists of an infinite number of mechanisms, that is to say, an infinite number of simplified systems may be established for research arising out of our ideas of organisation itself.

. . . . . .

In the study of the biology of man, the hypothesis has arisen that the nervous system forms the physiological substratum of consciousness. The more intensively the study of the nervous system has been pursued, the more its results have been found to support this hypothesis. It has, indeed, in the course of

time, led to the establishment of a great system of bold theoretical constructions, the various parts of which seem to give a scheme corresponding to the psychological elements and combinations of elements. "From the manifold combinations and ever advancing differentiation of originally quite simple reflex movements, physiology seeks to arrive at an understanding of the more complex activities of the higher central organs, right up to the conscious processes" (Natorp). This physiological system is the *material* for psychology, and psychology itself seeks to build up a corresponding system for the consciousness within.

From this alone it is clear that the ideas of "life" and "consciousness," so far as present knowledge goes, are not coincident. The commonest human experience has long since established this fundamental fact.

It is also clear that the *material* of psychology is derived, and entirely so, from experience, that is to say, from the science of nature, and that its method does not at any point operate with anything unknown to natural science.

We have further seen that even the "exact" sciences in their study of nature use psychological methods of the greatest importance, and that these form an indispensable step in every complete scientific method.

For the psychological side of any investigation is in every case something associated with the investigator himself, not with the object. It is only by hypothesis, by an idea of analogy, that he is able to ascribe consciousness to an object under investigation.

Thus it is through analogy that a human being assumes another consciousness, whether ascribed to another human being or to an animal. In this case it is a hypothetical construction, a system-arrangement, an idea of something which in principle corresponds to the *contents* of the investigator's own consciousness, and especially to the observations he has made. Modern comparative psychology is already fully at work upon the *collection of objective material* and upon the *construction on the basis of this*, of conceptions as to the content of consciousness in various animal forms.

One method of work, for instance, is that of ascertaining, by the most accurate anatomical and physiological investigations, the reflex channels, combining centres and corresponding reflexes of various orders. But in addition to this, there is also a branch of the science which bases its work upon observations in the field upon the "behaviour" of animals under varying circumstances. This method, of course, operates with more complex elements

than the foregoing, and has therefore been received with considerable scepticism in the world of science generally. It is also clear that the *judgment* of observations so made must be very difficult, in many cases uncertain, thus making special demands upon the accuracy of the observations and upon the planning of the work. But this ought not to be any ground for rejecting in principle the task of making observations in such a way. It is worth while to read what Hume says on this head (Human Understanding, Section IX.). Least of all should dogmatic (materialistic) science arrogate to itself the right of condemning such methods as being opposed to the great general conception of scientific method.

. . . . . .

Driesch[1] has sought to maintain that there is no logical difference between asking what things are, and asking what elements they represent in our understanding. From the critical point of view which I have endeavoured to follow in this sketch, there is, however, this difference, that with Kant's mode of formulating the question, we must recognise that we *never know more of things than science can tell us about them. Things are to us what science teaches us they are.* Science is im-

[1] *The Science of Philosophy of the Organism, loc. cit.*

perfect, though ever advancing. Yet science must for ever be the alpha and omega of philosophy.

. . . . . .

Contemporaneously with empiricism in science, we find highly developed industrialism in practical life. It is characteristic of the organisation of labour, according to this principle, that the work demanded of the workers makes few demands on mental activity. It is the age of a few leaders and a host of wage-earning workers. And just as empiricism ended in the dogmatic assertion that all science must follow empirical principles only, so also industrialism led to the belief that the final end of all social organisation must be the industrialisation of work and the dragooning of society into uniformity.

We have seen in this sketch that the sources of scientific method are especially derived from two different ages, the Renaissance and the transition period between the eighteenth and nineteenth centuries. In both these periods the work of mankind was organised in the hands of many independent workers. It was the age of skilled craftsmen, of the master-worker and his journeymen.

In our day, science has again succeeded in making possible an increasing development of

the power of independent individual activity. In science the individual worker meets problems which involve not only the collection and description of facts, but also the study of relations, the application of complicated methods, and the exercise of varied mental activities. In practical life the growth in the number of independent workers brought about by the invention of small machines and motors, the spread of education, and the development of co-operative systems, have given the personality of the worker a wider scope. The process commenced in the sphere of manual labour, and has made great strides in agriculture and in the fishing industry in many countries. Peculiar to this kind of work is the wide scope it allows for the use of various human faculties, the reflective and constructive as well as the observant and executive. In science itself, through the multiplication of laboratories, through participation in technology, in agriculture, in fishery work, in medicine, the scientist has likewise been led to occupy himself equally with thoughts and with concrete objects.

The satisfaction which this method of work is known to give, both to the scientific man and to other workers, the development of personality, the altered attitude towards the work itself, the substitution for a sense of compulsion of a sense

of enjoyment, render possible one of the highest *ideal* aims that have ever appeared to mankind, the development of this method in the future and its development into a *general* foundation for all kinds of work.

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